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Magic, Astrology, and the Early American Religious Heritage, 1600–1760

JON BUTLER

HISTORIANS HAVE ALWAYS TREATED America's earliest colonists as especially religious people. Some of the most distinguished scholarship in American history deals with the rise and fall of Puritanism, the religious awakenings of the colonial and early national periods, and the creation of the American evangelical style. Although impressive, these studies fail to comprehend the marrow of American religious life. They concentrate too fully, if brilliantly, on Christian theology, congregational life, and ecclesiastical polity, and they slight noninstitutional, popular religion. The low rate of church membership in the colonies reveals part of the problem. Contrary to assumptions underlying much of the work in American religious history, American colonists had an ambivalent relationship with Christian congregations. After about 1650 even in New England only about one-third of all adults ever belonged to a church. The rate was lower in the Middle and Southern colonies, and on the eve of the American Revolution only about 15 percent of all of the colonists probably belonged to any church.¹

Colonists also proved surprisingly ignorant of elemental Christian beliefs and practices. Naturally, clergymen worried when settlers failed to join their own sects or denominations. But even secular observers wondered at the number of settlers who ignored organized religious activity altogether. Colonial religious opinion embodied heterodoxy and sometimes simple, unprincipled confusion. In 1687 New York Governor Thomas Dongan wrote that

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¹ Edwin Scott Gaustad, *Historical Atlas of Religion in America* (New York, 1962), 158; Darrett B. Rutman, *Winthrop's Boston: Portrait of a Puritan Town, 1630–1649* (Chapel Hill, 1965), 145–48; Stephanie G. Wolf, *Urban Village: Population, Community, and Family Structure in Germantown, Pennsylvania, 1683–1800* (Princeton, 1976), 115–20; and Edward M. Cook, Jr., *The Fathers of the Towns: Leadership and Community Structure in Eighteenth-Century New England* (Baltimore, 1976), 119–41. Cook's work reveals sluggish progress toward church membership even among New England town officers.

settlers there usually expressed no religious sentiment at all or, when they did, entertained wildly unorthodox religious opinions. A New Jersey politician, Lewis Morris, observed in 1702 that in his colony, "except in two or three Towns, there is no face of any public worship of any sort, but people live very mean like Indians." In the 1760s the Anglican itinerant minister, Charles Woodmason, encountered Southern colonists who owned no Bibles, knew only enough about Christianity to ask that their children be baptized, and refused to join any church or support any minister. In his famous *Letters from an American Farmer* (1782), Hector St. John de Crèvecoeur remarked that in America the sects were so mixed and religious training so poor that "religious indifference is imperceptibly disseminated from one end of the continent to another, which is at present one of the strongest characteristics of the Americans."²

This discrepancy between popular colonial indifference to formal religious institutions and the early American reputation for deep religiosity reveals a contradiction in traditional American religious history. Unless the early American populace is viewed as irreligious, as some observers in fact thought it was, the numbers of settlers who were ambivalent toward institutional Christianity or ignored it altogether means that descriptions of common colonial religious practice simply cannot rest on histories of churches and church-related events. To understand what many colonists meant by religion, historians need to move beyond the study of ecclesiology, theology, and the ministry to recover noninstitutional religious practices. This task is difficult. Evidence about religion that is not linked to churches is extraordinarily difficult to find. In contrast, whole libraries are devoted to the histories of Christian groups. Even partial solutions to this problem will help us better comprehend the religious behavior of the great majority of colonial Americans rather than merely that of churches, ministers, and a few select laypersons. To that end I will examine the survival of European occult or magical practices in the American colonies, especially astrology, divination, and witchcraft. For American colonists were indeed religious, but many resorted to occult and magical practices unacceptable to most Christian clergymen and lawmakers. These practices declined after about 1720; explaining this decline forces a reconsideration of the role played by law and social elites in shaping the religious configuration of pre-Revolutionary America and a re-evaluation of the extent to which religious freedom developed in the colonies.

CUSTOMARILY, HISTORIANS DO NOT EXPLAIN what they mean by religion. But by not setting boundaries for discussion, as happens when church history and theology are allowed to portray religious belief in the society at large, narrow

² Woodmason, *The Carolina Backcountry on the Eve of the Revolution: The Journal and Other Writings of Charles Woodmason, Anglican Itinerant*, ed. Richard Hooker (Chapel Hill, 1954), 5–28; Morris, "The Memorial of Col. Morris concerning the State of Religion in the Jerseys," *Journal of the Society for the Propagation of the Gospel in Foreign Parts*, Appendix A, no. 2 (microfilm edition by Micro Methods, Ltd., Wakefield, Engl.); and Crèvecoeur, *Letters from an American Farmer* (New York, 1957), 47.

forms of religion can seem to represent all possible expressions. Of course, the literature on the philosophy of religion is vast and betrays scholarly disagreement rather than consensus. Yet, whenever historians write religious history, they implicitly choose one among several conflicting conceptualizations of religion, even when they fail to discuss the problem and to admit that alternative views might have produced different results.

I will use a reasonably traditional understanding of religion. Here “religion” means the resort to superhuman powers, sometimes beings, to determine the course of human events. In daily life each particular religion formulates an understanding about the nature of those powers and provides techniques its adherents can use to gain their support, protection, or aid in mastering life’s circumstances. Seventeenth- and eighteenth-century institutionalized Christianity obviously fits such a view of religion. But so, too, do numerous magical and occult arts—such as astrology, divination, and witchcraft—of the same period. Thus, magic and Christianity in colonial America were not generically different entities but were subsets of the same phenomenon—religion. They posited a resort to superhuman powers and they offered techniques for invoking those powers to control human events.³

Magic and Christianity also served remarkably similar ends for their adherents. To appreciate how close their ends were, Bronislaw Malinowski’s well-known model (followed by the great majority of historians of religion) that emphasizes their differences cannot stand. Malinowski treated Christianity as broad, philosophical, and ethereal and treated magic as narrow, practical, and mundane. Because these differences allegedly were so important, Malinowski insisted that Christianity was a “real” religious system but that magic was not.⁴ As the anthropologist Melford E. Spiro has pointed out, however, recent studies have failed to confirm Malinowski’s assertions for nonliterate societies. Nor does it work well for seventeenth- and eighteenth-century Europe or America. In neither pre-Revolutionary America nor the modern Third World is it clear that astrology, sorcery, and divination are only mundane and practical, while Christianity triumphs as philosophical and ethereal. Thus, if we want to understand the full range of religious expression in Western culture generally and early America specifically, it is imperative that we stop calling only the Christian or Judeo-Christian tradition “religious” and that we begin looking seriously at the place of magic and the occult arts in religious history.⁵

³ Melford E. Spiro, “Religion: Problems of Definition and Explanation,” in Michael Banton, ed., *Anthropological Approaches to the Study of Religion* (London, 1966), 85–126. The conceptualization of religion used here is a modified version of that developed by Spiro. It rejects the various “functionalist” approaches to religion utilized by theologian Paul Tillich, sociologist Peter Berger, or anthropologist Clifford Geertz, because as Spiro points out we can hardly say what religion “does” without first determining what religion is. For a debate on the ways historians define religion and magic that reaches conclusions different from those developed here, see Hildred Geertz and Keith Thomas, “An Anthropology of Religion and Magic, I and II,” *Journal of Interdisciplinary History*, 6 (1975–76): 71–109.

⁴ Malinowski, *Magic, Science, and Religion*, ed. Robert Redfield (Garden City, 1954), 88. Keith Thomas, in one of the most important books on religion in Tudor-Stuart England published in the last half-century, seems to have accepted Malinowski’s conceptualizations; see Thomas, *Religion and the Decline of Magic* (New York, 1971).

⁵ Spiro, “Religion: Problems of Definition and Explanation,” 87–94.

TRADITIONALLY, AMERICAN HISTORIANS have treated colonial religion as a story about the fate of Old World faith in New World society. "What happened to English Puritanism in America?" "Why did Dissenting churches prosper in the colonies when state-supported churches failed?" These inquiries have properly emphasized the transatlantic character of early America. But they have steered discussions of religion unnecessarily toward organized denominations and sects.⁶ How important it is to minimize the bias toward institutions and Christianity is seen in recent works on the religious configuration of early modern Europe, especially of England, that reveal an astounding variety of popular techniques to invoke superhuman powers and beings to master life and comprehend existence.⁷

On the eve of colonization Christianity in its different forms filled out only part of the English religious spectrum. Its adherents called on God's aid through prayer, through the intercession of the saints, or by preparing for the reign of Christ in the millennium. But occult practices comprised much of the remainder. In addition to witchcraft, these practices ranged from sophisticated arts such as astrology, chiromancy, geomancy, and metoposcopy (the reading of stars, palms, dots, and foreheads) to fortunetelling and divination (both highly idiosyncratic practices found in a bewildering variety of styles).⁸ Historians know most about occult religious practices among illiterate English folk society. But their range was far greater. Wealthy, prestigious men and women used and even promoted them as well. In London practitioners included Simon Forman and John Dee during the Elizabethan period, William Lilly under the Commonwealth, and William Salmon after the Restoration. Their clients often came from or worked for merchant and aristocratic families. Between 1645 and 1660 Lilly averaged over two thousand consultations per year. He and his colleagues wrote books and almanacs advancing their craft that were eagerly consumed by the reading public.⁹ Among England's illiterates, swarms of "cunning persons," "wise men," and "wise women" found an equally eager audience for magic and the occult. Popular practitioners manipulated what they believed were superhuman powers through a melange of astrological techniques, divination, and sorcery that remain difficult for the historian to decipher fully.

Because practitioners of white or good magic also were suspect as sources of black or bad magic, they often tended to get caught up in witchcraft prose-

⁶ Sydney Ahlstrom has, for example, concentrated on denominational history in a monumental study of institutionalized faith; see his *Religious History of the American People* (New Haven, 1972). For a superb review of Ahlstrom's work and the tradition it caps, see John Lankford, "An End and a Beginning: Reflections on Sydney Ahlstrom's 'Religious History of the American People' and the Future of Sociologically Informed Inquiry into Religion in American Life," *Anglican Theological Review*, 56 (1974): 463-81.

⁷ E. William Monter, *Witchcraft in France and Switzerland: The Borderlands during the Reformation* (Ithaca, N.Y., 1976); A. N. Galpern, *The Religion of the People in Sixteenth-Century Champagne* (Cambridge, 1976); Jean Delumeau, *Le Catholicisme entre Luther et Voltaire* (Paris, 1971); Wayne Shumaker, *The Occult Sciences in the Renaissance* (Berkeley and Los Angeles, 1972); and Emmanuel Le Roy Ladurie, *The Peasants of Languedoc*, trans. John Day (Urbana, 1974).

⁸ Thomas, *Religion and the Decline of Magic*, 212-44; and Alan Macfarlane, *Witchcraft in Tudor and Stuart England: A Regional and Comparative Study* (New York, 1970), 118-34.

⁹ A. L. Rowse, *Simon Forman: Sex and Society in Shakespeare's Age* (London, 1974), 14-36, 200-22; Derek Parker, *Familiar to All: William Lilly and Astrology in the Seventeenth Century* (London, 1975), 117-28; and Thomas, *Religion and the Decline of Magic*, 244-52, 300-22.

cutions. Yet no reputable study has demonstrated that practitioners of the occult—especially the illiterate cunning persons, wise men, and wise women—were organized in any way, and the records of witchcraft trials indicate the opposite. Accusers seldom described alleged witches as members of witch societies. In case after case victims testified that harm came to them after an altercation with an older person commonly thought to possess occult powers—usually an older, single woman. The witch was usually described as having cast a “spell” by cursing or swearing, not by ancient rituals. Witchcraft “worked” when the fear of occult power energized the frictions and anxieties of village society.¹⁰

Why did English men and women consult occult practitioners? The vast majority sought cures for sickness, injury, or other bodily afflictions. They believed they could be healed by the manipulation of superhuman forces. Thus, many seventeenth-century English medical writers—including Lilly, Salmon, Nicholas Culpeper, and Richard Saunders—prominently advertised their skill as astrologers. They linked the health of the body to the motions of the stars. The earth was thought to be a microcosm of the heavens, whose stars revealed the transcendent course of life. Horoscopes uncovered the individual’s relationship to the controlling forces of the universe. Using this or similar knowledge gained through divination, perceptive persons could utilize remedies made of astrally correct herbs and waters that would bring them into a positive, harmonious relationship with the universe and heal their afflictions. In fact, a parallel concern with medicine also helped advance some Christian groups in seventeenth-century England. Thousands of English men and women were drawn to the Quaker, George Fox, because he performed miraculous cures that he recorded in a now-lost manuscript “Book of Miracles.” Indeed, news of a cure Fox once performed in England, which one colonist “spread . . . up and down among the people in the country,” excited North Carolina settlers about Fox’s arrival there in 1672.¹¹ The public also kept practitioners busy with other tasks. Wise men and cunning persons found lost and stolen objects, clothing, household items, cattle, and children. They foretold the best day to sail, to begin a business venture, to marry, and to conceive children. Clerics consulted occult practitioners too. Simon Forman regularly saw the bishop of Rochester and other clerics hoping for richer parishes. In the 1640s William Lilly cast one horoscope for a minister who wished to know whether the Presbyterian system “would stand here in England” and another for a minister who wished to know whether he would

¹⁰ Macfarlane, *Witchcraft in Tudor and Stuart England*, 147–99; and Thomas, *Religion and the Decline of Magic*, 502–69. These studies have demolished the thesis advanced by Margaret A. Murray that witches in the Elizabethan period were part of an organized movement; see Murray, *The Witch Cult in Western Europe* (Oxford, 1921).

¹¹ Thomas, *Religion and the Decline of Magic*, 286–87, 295, 316–17; Rowse, *Simon Forman*, 58–61, 83, 98, 205; F. N. L. Poynter, “Nicholas Culpeper and the Paracelsians,” in Allen G. Debus, ed., *Science, Medicine, and Society in the Renaissance: Essays to Honor Walter Pagel*, 1 (New York, 1972): 201–20; Genevieve Miller, “A Seventeenth-Century Astrological Diagnosis,” in E. Asher Underwood, ed., *Science, Medicine, and History: Essays in the Evolution of Scientific Thought and Medical Practice Written in Honour of Charles Singer*, 2 (Oxford, 1953): 28–33; and George Fox, *Book of Miracles*, ed. Henry Cadbury (Cambridge, 1948), and *The Journal of George Fox*, ed. John L. Nickalls (rev. ed., Cambridge, 1952), 641–42.

ever preach to more affluent parishioners. Presumably, Lilly and his clerical clients thought astrology and divination would reveal what God still hid.¹²

How we understand relationships between occult religious practices and Christianity demonstrates how thoroughly questions shape answers. Christians everywhere believed that witchcraft existed, that it was part of the Devil's arsenal against Christ, and that it had to be crushed. Thus, the practice of witchcraft was illegal in all Catholic and Protestant countries, including England. Only a few "modern" writers like Johann Weyer in Germany or Reginald Scot and John Webster in England thought its existence was confined to biblical or early Christian times and that contemporaries who confessed to it were deluded.¹³ The relationship between other occult practices and Christianity is less clear. Occult practitioners sometimes linked them in crude ways. A seventeenth-century amulet that Keith Thomas has described was simply, even eloquently, inscribed "Jesus Christ for mercy sake, take away this toothache." The astrologer William Salmon saw little conflict between astrology and Christianity. He belonged to several Christian groups, yet he cast horoscopes and practiced astrologically based medicine in late Stuart London. Even Christians with impeccably Puritan credentials were sometimes reluctant to condemn certain occult practices. In 1656 an association of English Baptists asked "whether the practice of astrology in physick be lawful?" but decided to "wait upon the Lord for the light in this matter," although it did warn Baptists to be careful in their use of astrology in medicine.¹⁴ Literate and educated English men and women seeking spiritual insight through mystical revelation often drifted toward occult religious views and practices. Some turned to medieval Jewish mystical writings in the cabala to complement both their Christianity and their astrology. Some turned to Hermeticism, the study of the mystical and magical writings of a sixth-century Egyptian priest, Hermes Trismegistus. Some found truth in the healing theories and practices of the German physician, Paracelsus, and his disciple, Johann Baptiste van Helmont, who synthesized Christianity, alchemy, and Hermeticism. And still others, including Isaac Newton, immersed

¹² Rowse, *Simon Forman*, 129–70; and William Lilly, *Christian Astrology* (London, 1647), 432–34, 442.

¹³ Macfarlane, *Witchcraft in Tudor and Stuart England*, 14–22; Monter, *Witchcraft in France and Switzerland*, 32–36; Alfred Soman, "Witchcraft Trials in the Parlement of Paris, 1565–1640," in Dutch Group for the Study of the History of Crime and Criminal Law, *Proceedings* (Amsterdam, 1977), separately paginated; and H. C. Erik Midelfort, *Witch-Hunting in Southwestern Germany, 1562–1584: The Social and Intellectual Foundations* (Stanford, 1972), 25–29.

¹⁴ B. R. White, ed., *Association Records of the Particular Baptists of England, Wales, and Ireland to 1660*, 1 vol. in 3 pts. (London, 1971–73), 65; Rowse, *Simon Forman*, 129–70; and Thomas, *Religion and the Decline of Magic*, 180, 263–79, 371–85. An Anglican cleric, John Butler, cast horoscopes to demonstrate that December 25 was Christ's birth date and simultaneously advance the cause of astrology; see Butler, *Christologia: or, A Brief (but True) Account of the Certain Year, Month, Day, and Minute of the Birth of Jesus Christ* (London, 1671). Salmon apparently visited America and may have intended to settle there; he purchased land in South Carolina in 1685 but never farmed it. See Joseph I. Waring, *A History of Medicine in South Carolina, 1670–1825* (Charleston, 1964), 16. A pamphlet by Sebastian Smith attacked "Dr. S-lm-n" and claimed that Salmon returned from an undated trip to New England with a pregnant woman he subsequently refused to support or marry; see Smith, *The Religious Imposter: or, The Lives of Alexander, a Sham-Prophet, Doctor, and Fortune-Teller* (Amsterdam, n.d.).

themselves in alchemy, a craft whose discovery of ultimate truth was to be demonstrated by the transmutation of base metals into precious ones.¹⁵

The rediscovery of these occult religious practices offers a far different view of the spiritual legacy Europeans left to Americans from that most familiar to us. On the eve of colonization, many people in English society simply did not clearly divorce Christianity from magic, astrology, and divination. They adopted eclectic beliefs about superhuman powers and beings in their quest to understand the world and master their lives. Sometimes these beliefs incorporated Christian views, sometimes they did not. Little wonder, then, that the Puritan theologian, William Perkins, argued that occult practitioners challenged Christian ministers for the spiritual allegiance of the English public. “As the Ministers of God,” he wrote, “doe give resolution to the conscience, in matters doubtfull and difficult; so the Ministers of Satan, under the name of Wise-men, And Wise-women, are at hand, by his appointment, to resolve, direct and helpe ignorant and unsettled persons in cases of distraction, loss, or other outward calamities.”¹⁶ Thirty years later, John Gaule, author of *The Mag-Astro-Mancer: or, The Magical-Astrological-Diviner Posed and Puzzled* (1652), perceived a disturbing rise in the resort to occult religious practices. In those difficult times, he wrote, neither church nor state was “at leisure to examine and suppress it.” And in the same era a Puritan ministerial association in Cambridge warned the laity about spiritual imposters—not unrepentant Anglicans, as might be expected, but “witches, wizard[s], and fortune tellers.”¹⁷

DID OCCULT RELIGIOUS PRACTICES TAKE ROOT in America too? Their broad distribution throughout precolonial England might suggest that they did. Yet the character of early migration to America made the arrival of the full range of English occult practices unlikely and their survival difficult. The aims of prominent colonizers militated against occult religion; few colonial ventures were led by such self-conscious, narrow Christians as were the English colonies in America. As Perry Miller has argued, even Virginia exemplified the English Puritan desire to plant Christianity everywhere. The sociology of migration also struck at certain forms of occult religion. Although the peopling of early Virginia is difficult to discuss (so many early records have been lost), immigrants to New England before 1650 came from a relatively narrow sector of the English population—the gentry, the lesser gentry, and their sons

¹⁵ Joseph Blau, *The Christian Interpretation of the Cabala in the Renaissance* (New York, 1944); Frances Yates, *Giordano Bruno and the Hermetic Tradition* (London, 1964), and *The Rosicrucian Enlightenment* (London, 1974); Walter Pagel, *Paracelsus: An Introduction to Philosophical Medicine in the Era of the Renaissance* (New York, 1958); Allen G. Debus, *The English Paracelsians* (London, 1965); and Betty Jo Teeter Dobbs, *The Foundations of Newton's Alchemy: or, "The Hunting of the Greene Lyon"* (New York, 1975).

¹⁶ William Perkins, *A Discourse of the Damned Art of Witchcraft* (Cambridge, 1610), 3.

¹⁷ Gaule, *The Mag-Astro-Mancer: or, The Magicall-Astrological-Diviner Posed and Puzzled* (London, 1652), A2; and “Minutes of the Cambridge Classis,” in William A. Shaw, ed., *Minutes of the Bury Presbyterian Classis, 1647–1657*, in Chetham Society, *Remains Literary and Historical*, new ser., 41 (1898): 198.



Figure 1: Anonymous portrait of Johannes Kelpius, leader of a German pietistic community near the Wissahickon River just outside early Philadelphia, whose members accepted astrology as a legitimate Christian practice. Photograph courtesy of the Historical Society of Pennsylvania.

and daughters. Clearly, these people did not automatically reject occult religious practices. But their commanding dominance in the towns of New England and the virtual absence of immigrants from the poorest sectors of the English population did delay the arrival of the crudest forms of occult religion so important to the early Stuart religious experience.

Even the land militated against the occult. The English countryside abounded with places that men and women had long associated with magical happenings. By definition America lacked such places. It was a wilderness—not of itself or for the Indians who already lived there, but to its newest residents. Over many decades, nevertheless, these settlers did invest the landscape with sacred, even magical, meaning.¹⁸ Although the use of magic never prospered here as it had in England, a wide range of occult ideas and practices—alchemical practitioners, the availability of occult books, occult notions in colonial almanacs, and even a few self-announced cunning persons—were evident in the colonies between 1650 and 1720.

Several New Englanders well known in English alchemical circles pursued alchemical studies. Robert Childe and George Stirk both practiced alchemy while in America in the 1630s and 1640s and continued to do so after they returned to England during the Civil War. John Winthrop, Jr., eldest son of the Bay Colony's most famous governor, brought an enormous alchemical library to America. He may also have been the author of major alchemical treatises published under the pseudonym Eirenaeus Philaletha. Widely circulated in England and Europe, these tracts were translated into Italian in 1695. Since the works clearly were written by someone who spent considerable time in the colonies, even if the claim for Winthrop's authorship is put aside, it seems likely that Americans not only read sophisticated occult literature but contributed to it as well.¹⁹

Some occult religious practices came from non-English European immigrants. In 1694 Johannes Kelpius, a German mystic, established a settlement outside Philadelphia called the "Woman of the Wilderness," which later became part of a much larger communitarian settlement at Ephrata. The spiritual focus of these settlements was Christian. But their utilization of astrology, knowledge of Hermeticism, involvement with the secret Rosicrucian movement (the adherents of which combined Christianity with Hermeticism) and their apparent willingness to cast horoscopes separated them from other Christian groups in the Delaware valley. Although their influence is difficult to measure, a few of Johannes Kelpius's surviving letters to settlers in Rhode

¹⁸ Perry Miller, "Religion and Society in the Early Literature of Virginia," *William and Mary Quarterly*, 3d ser., 5 (1948): 492–522, and 6 (1949): 24–41; and T. H. Breen and Stephen Foster, "Moving to the New World: The Character of Early Massachusetts Migration," *ibid.*, 30 (1973): 189–222.

¹⁹ Harold Jantz, "America's First Cosmopolitan," *Proceedings of the Massachusetts Historical Society*, 84 (1972): 3–25; Robert C. Black III, *The Younger John Winthrop* (New York, 1966), 87–88; and Ronald S. Wilkinson, "'Hermes Christianus': John Winthrop, Jr., and Chemical Medicine in Seventeenth-Century New England," in Debus, *Science, Medicine, and Society in the Renaissance*, 222–41, "The Alchemical Library of John Winthrop, Jr. (1606–1676) and His Descendants in Colonial America," *Ambix*, 11 (1963): 33–51, and 13 (1965–66): 139–86, and "George Starkey, Physician and Alchemist," *ibid.*, 11 (1963): 121–52.

MEDICINA MAGICA

TAMEN PHYSICA:

Magical, but Natural Physick.

O R

A Methodical Treatise of *Diastatical Physick.*

Containing the general Cures of
all infirmities : And of the most radi-
cal, fixed, and malignant Diseases
belonging, not only to the Body of Man,
but to all other Animal and Dome-
stic creatures whatsoever, and
that by way of

TRANSPLANTATION.

With a Description of a most ex-
cellent Cordial out of Gold,
much to be estimated.

Published by *Samuel Boulton, Salop,*

Ar non habet inimicum nisi ignorantem.

London, Printed by T. C. for N. Brook, at
the Angel in Cornhill, 1765

Figure 2: The Reverend Thomas Teackle of Accomac County, Virginia, owned this book by Samuel Boulton as well as all the books on hermetic medicine Boulton recommended to his readers. Photograph courtesy of the Owen H. Wangenstein Historical Library of Medicine and Biology, University of Minnesota.

Island and Virginia suggest that neither distance nor ethnic differences prevented colonists from seeking information on occult Christianity.²⁰

Library lists from late seventeenth- and early eighteenth-century Virginia estate inventories point to the widespread ownership of occult books in the American colonies. The lists do not indicate how or why such books were acquired. Nor do they indicate what their owners did with them or, for that matter, with any Bibles they owned. But they do indicate that some cared to buy and keep occult books with surprising frequency. The occult books mentioned most often in the inventories were the popular medical treatises by astrologer-physicians like Nicholas Culpeper and William Salmon. Even small

²⁰ Julius F. Sachse, ed., "The Diarum of Magister Johannes Kelpius," in Pennsylvania German Society, *Proceedings and Addresses*, 25 (1914-15): separately paginated, and *The German Pietists of Provincial Pennsylvania, 1694-1708* (Philadelphia, 1895), 13-27, 125-38, 219-50.

libraries, however, held other occult books. At his death in 1701, Ralph Wormley of Middlesex County, a prominent Anglican, owned not only treatises by John Webster and John Gaule that attacked occult religion and witchcraft but also books that defended and advanced occult practices: John Baptiste Porta's *Natural Magick* (1658); the collected works of a German alchemist named Johann Glauber; three works by the Stuart vegetarian, astrologer, and Christian, Thomas Tryon—*Pythagoras His Mystic Philosophy Revived* (1691), *A New Art of Brewing Beer, Ale, and Other Sorts of Liquors* (1690), and *The Way to Health, Long Life, and Happiness: or, A Discourse of Temperance* (1697); and Richard Mathew's popular distillation of alchemical healing secrets, *The Unlearned Alchymist His Antidote* (1662). Mathew Hubbard, who died in 1667, owned Joseph Moxon's *A Tutor to Astronomie* (1659). This book instructed readers in astrology as well as in astronomy and was reprinted many times. Richard Lee, who died in 1715, owned a William Lilly almanac of the 1650s, the alchemical works of Albertus Magnus, Vincent Wing's astrological and astronomical *Urania Practica* (1649), and a Latin edition of Wolfgang Hildebrand's *Magia Naturalis* (1610), a German occult work. And Edmund Berkeley, a former member of the Virginia Council who died in 1718, owned a book his estate inventory listed as "Physiognomy and Chiromancy," which was probably the text of the same title that the astrologer Richard Saunders published twice (1653 and 1671) in the seventeenth century.²¹ The library of Thomas Teackle, an Anglican minister, was especially astounding for the range of its occult titles. Unfortunately, little is known about Teackle except that he served as a parish priest in Accomac County, Virginia from the mid-1650s to his death in 1696 or 1697. His library was also notable for its collection of Puritan works, yet another sign of the heterodox character of seventeenth-century Virginia Anglicanism. But its large number of occult works also suggests that Teackle may have been one of those mysterious and little-known Europeans whom Frances Yates has described as defenders of hermetic and occult religious truth at the end of the seventeenth century.²²

The sophistication of Teackle's occult book collection is in part demonstrated by the way it followed instructions given in Samuel Boulton's treatise synthesizing Hermeticism, Paracelsian medicine, and Christianity—*Medicina Magica Tamen Physica: Magical but Natural Physick* (1656)—a book Teackle owned. Boulton suggested that serious readers could make more effective use of his own work by acquiring three additional volumes for reference. These were Oswald Croll's synopsis of Paracelsian medicine, *Bazilica Chymica* (1670), a frequently reprinted sixteenth-century Italian collection of occult remedies

²¹ For lists of these books, see the *William and Mary Quarterly*, 1st ser., 2 (1893–94): 169–74; *ibid.*, 3 (1894–95): 44–45, 133–34; and *ibid.*, 7 (1899–1900): 18–19, 230. Two persons important to the history of the American colonies, William Penn and Francis Nicholson, subscribed to the English edition of an avowedly occult work that synthesized Christianity, Hermeticism, and alchemy; see Christopher Packe, trans., *The Works of John Rudolph Glauber . . .* (London, 1689).

²² Yates, *Giordano Bruno and the Hermetic Tradition*, 416–31, and *The Rosicrucian Enlightenment*, 235–63; and Philip A. Bruce, *Institutional History of Virginia in the Seventeenth Century*, 1 (New York, 1910): 150, 175–76, 182–83, 211.

by Levinus Lemnius, *De Miraculis Occultis Naturae* (1559), and one of the books also owned by Ralph Wormley, Porta's *Natural Magick*. In fact, Teackle probably owned all three books. He owned two copies of Croll's work, a Latin edition of Lemnius's work, and a book his estate inventory lists as "Magica Nature, etc., an old Book with a parchment cover, the Title page torn out." This probably was either Porta's *Natural Magick* or Wolfgang Hildebrand's *Magia Naturalis*, which Richard Lee also owned.²³ Teackle owned other occult and astrological medical works: Anthony Askham, *A Little Herbal* (1550); Johannes Beverovicus, *Epistolica quaestio de vitae termino* (1634); Joseph Du Chesne, *Quercetanus Redivivus, hoc est are medica dogmatica-hermetica, ex scriptus J. Quercetani* (1679); Johann Baptista van Helmont, *Oriatrike: or, Physick Refined*, trans. John Chandler (1662); and Johann Schroder, *Pharmacopeia Medico-Chymica, sive Thesaurus Pharmacologicus, quo composita quaeque celebriosa* (1644). Teackle possessed an encyclopedia of occult knowledge in his copy of William Salmon's *Medicina Practica: or, Practical Physick* (1692), containing scores of astrologically based recipes for cures, a synopsis of the philosophy of Hermes Trismegistus, and four hundred pages of medieval and early modern European alchemical lore.²⁴

Three books Teackle owned stemmed from the occult Rosicrucian movement. Thomas Vaughan's *Magia Adamica: or, The Antiquitie of Magic and the Descent thereof from Adam Downwards* (1650) and John Heydon's *The Rosi Crucian Infallible Axiomata: or, General Rules to Know All Things Past, Present, and to Come* (1660) were English efforts to advance the cause of Christian Hermeticism. Teackle also owned one of several works of the Jesuit, Athanasius Kircher, who interpreted Hermes Trismegistus in the light of Egyptian hieroglyphs.²⁵ Finally, Teackle owned three other occult treatises: Marin Cureau de la Chambre's *The Art How to Know Men* (1670), which analyzed human passions and explained how to use astrology, chiromancy, and metoposcopy to predict the future; Guilielmus Gratarolus's sixteenth-century collection of alchemical texts, *Varae Alchemiae Artisq̃ue Metallicae, citra Aenigmata* (1561); and Johann Reuchlin's *De Arte Cabalistica*, a Renaissance exploration of Jewish mysticism that was a favorite text of intellectuals seeking universal religious truth in post-Reformation Europe.²⁶ Again, that Teackle owned these books says nothing about how he used them. But his library testifies to the availability of occult books in America and to the ability of the colonists to assemble an occult library despite their distance from Europe.

Colonial almanacs also were important sources of occult religious concepts. American historians have often observed that almanacs outsold the Bible and

²³ Samuel Boulton, *Medicina Magica Tamen Physica: Magical but Natural Physick* (London, 1656), 117–19, 125. Teackle's ownership of two copies of Croll's work is revealed by its double appearance on the carefully drawn inventory, once under its main title, *Bazilica Chymica*, and once under its subtitle, *Royal and Practical Chemistry*.

²⁴ Teackle also owned two books that attacked occult medicine: Walter Charleton, *Physiologia Epicuro-Gassendo-Charletomana: or, A Fabric of Science Natural* (London, 1654); and Robert Sprackling, *Medela Ignorantiae* (London, 1665).

²⁵ Yates, *Giordano Bruno and the Hermetic Tradition*, 416–23.

²⁶ Blau, *Christian Interpretation of the Cabala in the Renaissance*, 41–64.

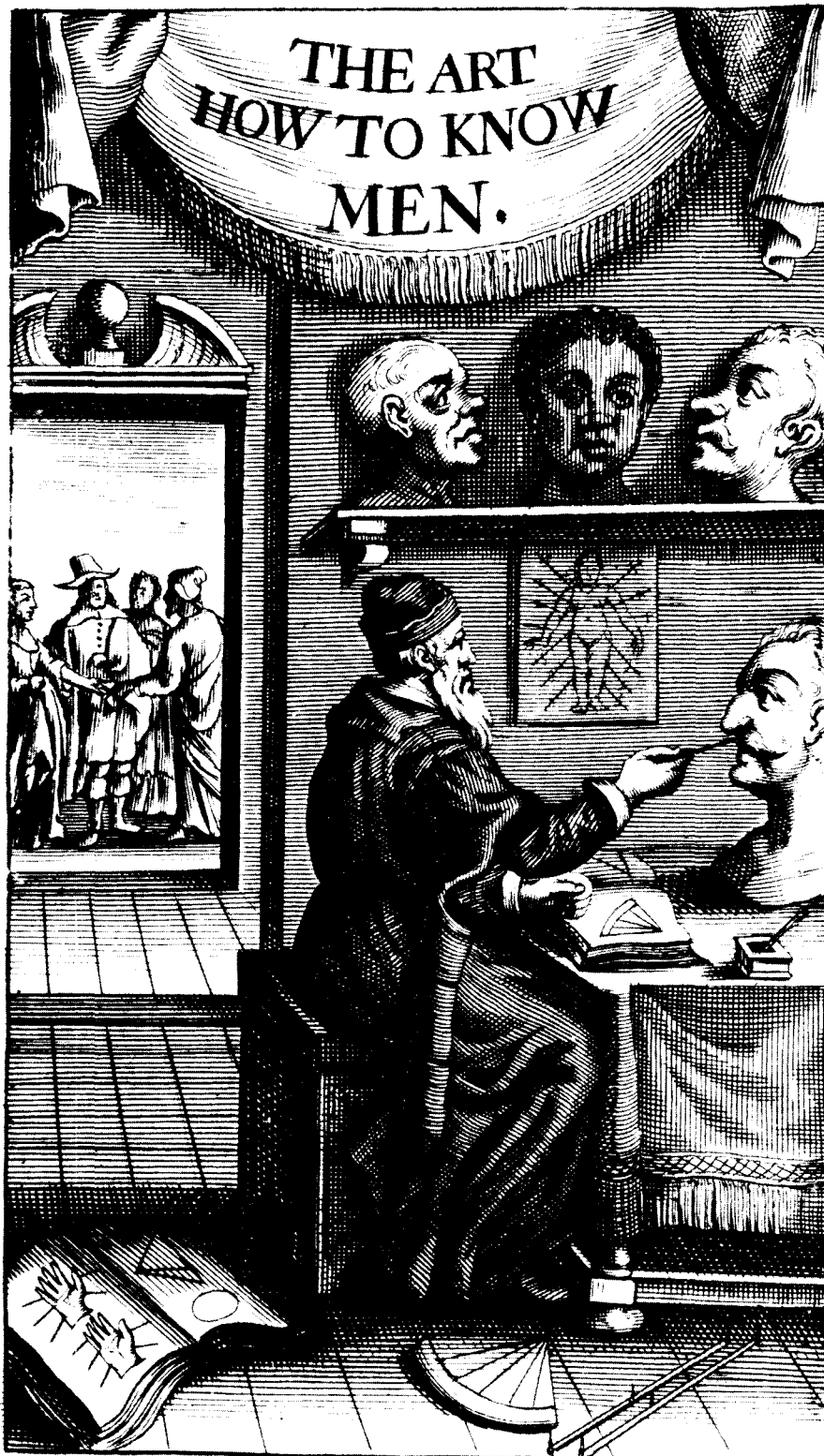


Figure 3: The frontispiece of Marin Cureau de la Chambre's *The Art How to Know Men* (1670), a compilation of instructions in occult crafts that was owned by the Virginia Anglican minister, Thomas Teackle. Photograph courtesy of the Newberry Library, Chicago.

were the most popular book publications in the colonies. Yet the meaning of their popularity—usually mentioned only as an anecdote—has never been made clear. Like their English counterparts, colonial almanacs provided all of the astronomical information—contained in a twelve-month calendar called the “ephemerides”—necessary to make astrological calculations. The “ephemerides” calculated positions of the stars and planets for the locale in which the almanac was sold. With its help readers could cast horoscopes to correlate their activity with the motions of the heavens. Colonial almanacs usually included the crude figure of a man, called the “anatomy,” circled by signs of the zodiac that pointed to the parts of the body that each controlled. Even semiliterate readers could use the illustration to match astrally correct herbs with troublesome diseases and thus effect cures for themselves or others. Almanacs informed readers about good and bad days for bleeding. These judgments were based on calculations tied to phases of the moon. In a list called the “vulgar notes,” almanacs also included those numbers, letters, and days thought to have magical, superhuman significance for the coming year. And they made some attempt to engage in “judicial astrology,” by predicting the future.²⁷

The popularity of the almanacs was linked directly to their occult contents. Colonists insisted that almanacs contain occult material. No one knew this better than those who compiled almanacs. These men complained bitterly about their customers’ demands. In his 1728 almanac, Nathaniel Ames of Dedham, Massachusetts reprinted a poem from a 1633 English almanac that protested the pressure the public placed on the compilers to print the anatomy. Ames also complained that even readers who did not understand the anatomy demanded its inclusion, and he printed a poem taken from Samuel Clough’s *New York Almanac* of 1703, that complained again about purchasers’ demands to see an anatomy in each year’s almanac:

The Anatomy must still be in,
Else the Almanack’s not worth a pin,
For Country-men regard the Sign,
As though ‘Twere Oracle Divine.²⁸

Almanac-makers complained that readers demanded too much in the way of predictions of the future. Pennsylvania’s Daniel Leeds, who in 1698 defended the general proposition that lunar and solar eclipses affected world politics, wrote a year earlier in his almanac that he yet dared “not assign and limit their effects to any particular place” because readers misused the information or were “too much led astray in these latter Ages by many of their Teachers.”

²⁷ Herbert Leventhal, *In the Shadow of the Enlightenment: Occultism and Renaissance Science in Eighteenth-Century America* (New York, 1976), 22–23. Although no full scholarly study of American almanacs yet exists, three works introduce the topic: Leventhal, *In the Shadow of the Enlightenment*, 22–27, 32–36, 38–39, 47–56; George L. Kittredge, *The Old Farmer and His Almanac* (Boston, 1904); and Marion B. Stowell, *Early American Almanacs: The Colonial Weekday Bible* (New York, 1977). For a model European study, see Genevieve Bolleme, *Les almanachs populaires aux XVII^e et XVIII^e siècles: Essai d’histoire sociale* (Paris, 1969).

²⁸ Nathaniel Ames, *An Astronomical Diary: or, An Almanack for . . . 1728* (Boston, 1728).

In 1743 a bitter Jacob Taylor described in disgust the American colonists' use of almanacs rather than Bibles to solve personal crises through occult means: "Billy and Dicky, Peggy and Molly must see the Man on the Moon; and when the little child cries the great one runs for the Almanack, to bless the House with Peace." Little wonder that John Jerman, a Pennsylvania almanac-maker, described the almanac as the prisoner rather than master of its consumer.

I During Life a Servant am, and I
Just one poor Year shall live that I must die;
Am sold for five pence; and obliged to go,
A Slave, compelled to serve I know not who.²⁹

Such protests make it difficult to discern whether individual almanac-makers were deeply committed to occult religious practices themselves or just printed what they believed their buyers wanted. Some, however, obviously knew occult literature well. Before 1710 Pennsylvania's Jacob Taylor quoted Hermes Trismegistus and Cornelius Agrippa, a Renaissance defender of some occult practices, in his almanacs. He even found a quotation from Sir Francis Bacon—"though Fortune be blind yet she is not invisible"—to support the belief that the movement of the heavens was important to people. At the same time he attacked the crude occult arts of "Necromancy, Exorcism," and sorcery. Another Pennsylvanian, Daniel Leeds, was even more enthusiastic. Disowned by the Quakers in 1688, Leeds subsequently defended what he described as true religion from the corruption of organized denominations and sects. Even earlier he had demonstrated his knowledge of mystical Christianity by publishing extracts from the work of the German mystic, Jacob Boehme. Later, in his almanacs, he quoted Hermes Trismegistus, summarized seventeenth-century astrological predictions of William Lilly and John Partridge, trumpeted Paracelsian medicine at a time when its influence had waned dramatically, quoted Psalms 78:49 to support the notion that the "first Cause" (who used angels as agents) also used stars as "second Causes of Effects upon Mankind," and reminded readers to gather herbs only "when the Planets that govern them are dignified and friendly aspected."³⁰

Evidence drawn from the Salem witchcraft trials suggests that the colonists' ownership of occult books and almanacs also betrays some real resort to malevolent occult practices. In his *Witchcraft at Salem* Chadwick Hansen has argued that several persons tried at Salem had probably practiced white magic earlier and may have dabbled in black magic as well. Dorcas Hoar was known as a fortuneteller in the 1680s. In her 1692 trial she described a book on

²⁹ Daniel Leeds, *An Almanack for . . . 1697* (New York, 1697); Leeds, *An Almanack for . . . 1698* (New York, 1698); Jacob Taylor, *An Almanack for 1743* (Philadelphia, 1743), quoted in Leventhal, *In the Shadow of the Enlightenment*, 36; John Jerman [*The American Almanack for . . . 1736* (Philadelphia, 1736)]; and Gilbert Cope, "Jacob Taylor, Almanac Maker," *Proceedings of the Chester County Historical Society* (1908), 10–28.

³⁰ Jacob Taylor, *An Almanack for . . . 1705* (Philadelphia, 1705), and *Ephemeris Sideralis: or, An Almanack for 1707* (Philadelphia, 1707); and D[aniel] L[Leeds], *The Temple of Wisdom for the Little World* (Philadelphia, 1688), 1–125; *An Almanack and Ephemerides for . . . 1693* (Philadelphia, 1693), *An Almanack for . . . 1695* (Philadelphia, 1695), and *An Almanack for . . . 1697* (Philadelphia, 1697).

chiromancy with an accuracy that suggests she used, and perhaps owned, it. Goody Bishop owned rag dolls stuck with pins and could not satisfactorily explain why she kept them. A black or Indian slave, Candy, produced rags, grass, and cheese that she used as mediums through which she attempted to harm others. And several witnesses reported that a Wilmot “Mammy” Red cursed a Mrs. Simms of Salem Town in an attempt to use occult means to prevent Mrs. Simms from urinating.³¹

The Salem trials also lift up otherwise neglected aspects of Cotton Mather’s views on the popularity of occult practices. Two years before the Salem trials, Mather was so concerned about the number of settlers who used occult techniques for curing illnesses and settling quarrels that he described the Christian defense against them in occult terms—as “amulets”—so readers could more readily understand him. In a sermon delivered in 1689 Mather acknowledged that New Englanders had many occult practices available to them, including witchcraft, enchantments, and charms. But he assured even weak Christians that they were not defenseless against occult practices and the temptation to use them: “[T]here are three admirable *Amulets* that I can heartily recommend unto you all: *A fervent Prayer . . . A Lively Faith . . . [and] A Holy Life*”:

Use these things as the Shields of the Lord. . . . Suppose now that any *Witches* may let fly their Curses at you, you are now, like a *Bird* on the *Wing*, in such Heavenward Motions that they cannot hit you. Now the *Devils* and their Creatures cannot say of you, as the *Demon* said of the Christian Woman whom, at a *Stage-play* he took *Possession* of, and being asked, gave this reason of his taking her, *I found her on my own ground*.³²

Thirty years later Mather still was concerned about the use of occult remedies in New England. In his *The Angel of Bethesda*, a medical work written around 1720, Mather claimed that unfulfilled hopes for good health drove colonists to occult religion and, hence, away from God. Colonists hoped for “some Ease of their Distempers” and consulted occult practitioners. They knew the occult remedies had “no Natural Efficacy for the Cure of Diseases” and, according to Mather, therefore realized that they were seeking cures from the Devil, not from God.³³

Evidence from Virginia and Pennsylvania fills out the picture of occult practices in the colonies. With regard to religion in the first American colony, Darrett B. Rutman has written,

³¹ Chadwick Hansen, *Witchcraft at Salem* (New York, 1969), 93–103. John Demos has warned historians against using the court record to determine whether any of the accused really practiced witchcraft and has suggested that the question probably can never be answered. Despite the problem of finding evidence that does not descend from courts or Christian ministers, the present essay is designed to demonstrate that historians can, in fact, uncover actual occult practices, including witchcraft. Demos, “Underlying Themes in the Witchcraft of Seventeenth-Century New England,” *AHR*, 75 (1969–70): 1311–26, and “John Godfrey and His Neighbors: Witchcraft and the Social Web in Colonial Massachusetts,” *William and Mary Quarterly*, 3d ser., 33 (1976): 242–65.

³² Mather, “A Discourse on Witchcraft,” in *Memorable Providences, Relating to Witchcrafts and Possessions* (Boston, 1689), 19–21 (Mather’s emphasis).

³³ Mather, *The Angel of Bethesda*, ed. Gordon W. Jones (Barre, Mass., 1972), 293–301.

In sound mind and with clear conscience a Virginian could account his poor hunting to the spell of another (1626), could hold that only the horseshoe over his door protected his sick wife from the evil intentions of a neighbor woman who perforce passed under it on her way to saying black prayers at his wife's bedside (1671), could attribute to a witch the death of his pigs and withering of his cotton (1698), and, in court, faced with suits for slander, could insist that "to his thoughts, apprehension or best knowledge" two witches "had rid him along the Seaside and home to his own house" (again 1698).³⁴

Church and court records reveal deeply rooted occult practices in Chester County, Pennsylvania, a farming area south of Philadelphia. In 1695 the county's Quaker leaders demanded that Phillip Roman, Jr. and his brother Robert stop using astrology, geomancy, and chiromancy "in resolving questions Concerning Loss and gain with other vain Questions." After much argument Phillip agreed. He told the Quaker monthly meeting that "he had denied Severall that came to him to be resolved of their questions already." But Robert clung to his occult crafts. Although he agreed to abandon some, he insisted on practicing others if "it was to do some great good." Since Quaker leaders "could not remove him" from this position, they disowned him and circulated an epistle to Pennsylvania and New Jersey Quaker meetings denouncing those "who, professing the art of astrology, have undertaken thereby to give answers and astrological judgments concerning persons and things." Having failed to use church discipline successfully to change Robert Roman's behavior, Chester County's Quaker leaders turned to the legal institutions they now commanded in America to stop the younger Roman's practices. Since many of them sat as county magistrates, they indicted Roman "for practicing geomancy according to hidon and divineing by a sticke" and for using occult means to take "the wife of Henry Hastings away from her husband and children." To support their accusations, they named three books they found in Roman's possession: "Hidon's Temple of Wisdom which teaches geomancy, and Scots Discovery of witchcraft, and Cornelias Agrippas Teaching negromancy [*sic*]." John Heydon's *Theomagia: or, The Temple of Wisdom* (1644) and a book falsely attributed to Agrippa, *The Fourth Book of Occult Magic* (1655), were standard occult texts. But Reginald Scot's *Discoverie of Witchcraft* (1584) was an Elizabethan denunciation of witchcraft trials themselves. Scot argued that in modern times witches simply did not exist. The attribution mistaken to Agrippa by the Chester County magistrates is a superb reflection of their concern for the extent of occult religious practice in the county. In any case, the court fined Roman £5 and ordered that he "never practise the arts but but [*sic*] behave himselfe well for the future."³⁵

³⁴ Rutman, "The Evolution of the Religious Life of Early Virginia," *Lex et Scientia: Journal of the American Academy of Law and Science* (forthcoming); and Richard Beale Davis, "The Devil in Virginia in the Seventeenth Century," in Davis, *Literature and Society in Early Virginia, 1608–1840* (Baton Rouge, 1973), 14–41.

³⁵ Minutes, Concord Monthly Meeting, 11–9 mo.–1695, 9–10 mo.–1695, 13–11 mo.–1695/6, 9–1 mo.–1696; and Minutes, Concord Quarterly Meeting, 3–12 mo.–1695 (microfilm copies at Friends Historical Library, Swarthmore College, Swarthmore, Pa.). Amelia M. Gummere, *Witchcraft and Quakerism: A Study in Social History* (Philadelphia, 1908), 46–47; Ezra Michener, *Retrospect of Early Quakerism: Being Extracts from the Records of Philadelphia Yearly Meeting and the Meetings Composing It* (Philadelphia, 1860), 364–67; and *Records of*

Two additional Chester County court cases offer more direct evidence that cunning persons practiced there. In September 1696 the magistrates tried James Woodward for stealing a thimble and bottle of rum from James Swaford. Swaford testified in court that Woodward said the missing goods could be found by an occult practitioner. Woodward allegedly told Swaford “he would goe to a wise man a bout them and After some time his son Richard came to me and would have me to meet him Att a wise man’s house and there I should hear of my goods.” After giving the proposition some thought—a telling point in itself—Swaford rejected it. “I answered I would not goe.”³⁶ Although Robert Roman might have been the cunning person Swaford could have visited, testimony in a case prosecuted two years later suggests that more than one wise man lived in the county. In 1698 Chester County magistrates cited Richard Crosby for contempt after he called one magistrate’s relative a “whore” and jeered the rest of them as “knaves” and “rogues.” During his contempt hearing Crosby contrasted the magistrates’ ignorance of occult skills with his own occult knowledge, “saying he and his son knew more of the mathematical arts then any of us all, pretending he could tell fortunes, and who had stole goods and describe where they were.” Unfortunately, no further evidence of Crosby’s skills appears in later records, although Crosby continued to appear before the magistrates on disorderly conduct charges for another decade.³⁷

WHAT HAPPENED TO THE ENGLISH OCCULT TRADITION in early America? By most accounts it declined and died. The rise of Enlightenment philosophy, scepticism, and experimental science, the spread of evangelical Christianity, continuing opposition from English Protestant denominations, a rise in literacy closely associated with Christian catechizing, and the emergence of the colonies as modern cultural, economic, and political entities all have led historians to discount the significance of occult religious practices in the last years of the colonial period. Yet some evidence, at least, points toward their partial survival in eighteenth-century America. The astronomical information in almanacs always could be used to make astrological calculations even if the almanac-maker did not intend it to serve that purpose. On the eve of the Revolution some almanacs still printed the anatomy and listed the days when bleeding was good or bad. Occult techniques of physiognomy and chi-

the Courts of Chester County Pennsylvania, 1681–1697 (Philadelphia, 1910), 363, 371. Although the term “negromancy” could have carried racial overtones in the colonial period, the point is difficult to establish. Seventeenth-century English writers used the term interchangeably with “necromancy,” without apparent racial connotations; see, for example, *A Plot lately Discovered for the Taking of the Tower by Negromancie* (London, 1641). Although the term “negromancy” might have acquired racial connotations in America as slavery proliferated, the evidence, if so, does not yet indicate where or when.

³⁶ *Records of the Courts of Chester County Pennsylvania, 1681–1697*, 393.

³⁷ *Ibid.*, 398, 405, 406; *Records of the Courts of Chester County Pennsylvania* [2] (Danboro, Pa., 1972), 2, 3, 126, 160; and Herbert W. K. Fitzroy, “Richard Crosby Goes to Court, 1683–1697,” *Pennsylvania Magazine of History and Biography*, 62 (1938): 12–19. For the extraordinary religious turmoil of Chester County in the 1690s, see my “Into Pennsylvania’s Spiritual Abyss: The Rise and Fall of the Later Keithians, 1693–1703,” *ibid.*, 101 (1977): 151–70.

romancy could be gleaned from at least some medical books. In America the best source was *Aristotle's Master Piece*, a book also renowned for its lurid descriptions of uncommon sexual maladies.³⁸ In philosophy, a recent study by Herbert Leventhal describes the widespread survival of occult ideas among eighteenth-century intellectuals. The notion that stars influenced and at least reflected human activity still was relatively popular in the 1730s. Yale's president, Ezra Stiles, and a southern surveyor and botanist, John W. G. DeBrahm, rediscovered alchemy after it had virtually died out elsewhere. Through a phenomenon called "rattlesnake gazing" colonists even added to the occult beliefs they had brought to America from Europe. The snake allegedly overpowered humans with its eyes, and the elaboration of this concept in pre-Revolutionary America testifies to the perseverance of a specifically occult concept of the unity of man, animals, and nature and, hence, to the ability of one to manipulate the other.³⁹

Even fear of witchcraft continued. True, the Salem episode of 1691–92 produced not only the last witch executions in the English-speaking world but also many denunciations of the beliefs that produced them. Yet in 1705 two Virginia juries examined Grace Sherwood for witches' marks. They reported that her body was "not like them nor noe other woman that they knew of, haveing two things like titts on her private parts." When the court subjected her to the infamous dunking test, she unfortunately swam ("Contrary to Custom") and she was therefore bound over for trial on charges of witchcraft. Her fate, however, remains unknown because the court's records have been lost. A year later in 1706 the chief justice of South Carolina, Nicholas Trott, delivered an impressive charge to a Charleston grand jury imploring it to prosecute witches. He refuted attacks made on the prosecution of witches by two English writers, Reginald Scot and John Webster, and backed his own demand for such prosecutions with quotations from early Church fathers and Cotton Mather.⁴⁰

South Carolina also furnished the only known manuscript of occult cures to survive from seventeenth- and eighteenth-century America, the so-called Joshua Gordon "Witchcraft Book" in the South Caroliniana Library, University of South Carolina, Columbia. Nothing is known about its compiler. But whoever he was, he took great care to make a ritual-like arrangement of its contents. Two pages of the kind of charms common to the English occult tradition open and close the volume. On the first page the phrase "Behold

³⁸ Leventhal, *In the Shadow of the Enlightenment*, 38–39, 262–64; and William D. Stahlman, "Astrology in Colonial America: An Extended Inquiry," *William and Mary Quarterly*, 3d ser., 13 (1956): 551–63. Also see Otho T. Beall, Jr., "Aristotle's Master Piece in America: A Landmark in the Folklore of Medicine," *ibid.*, 20 (1963): 207–22. The 1755 edition of this work contained an introduction to chiromancy and a description "Of the Powers of the Celestial Bodies over Men and Women"; *Aristotle's Master Piece*, 96–119, 126–29.

³⁹ Leventhal, *In the Shadow of the Enlightenment*, 126–67.

⁴⁰ Edward W. James, "Grace Sherwood, The Virginia Witch," *William and Mary Quarterly*, 1st ser., 3 (1894–95): 96–101, 190–92, 243–44, and 4 (1895–96): 18–22; and Nicholas Trott, "A Charge Delivered at the General Sessions . . . 1705/06," in L. Lynn Hogue, "An Edition of 'Eight Charges Delivered, at so Many Several General Sessions . . . [1703–1707] by Nicholas Trott'" (Ph.D. dissertation, University of Tennessee, 1972), 133–63. Michael Hindus of the Harvard Law School kindly drew my attention to Trott's charges concerning witchcraft.

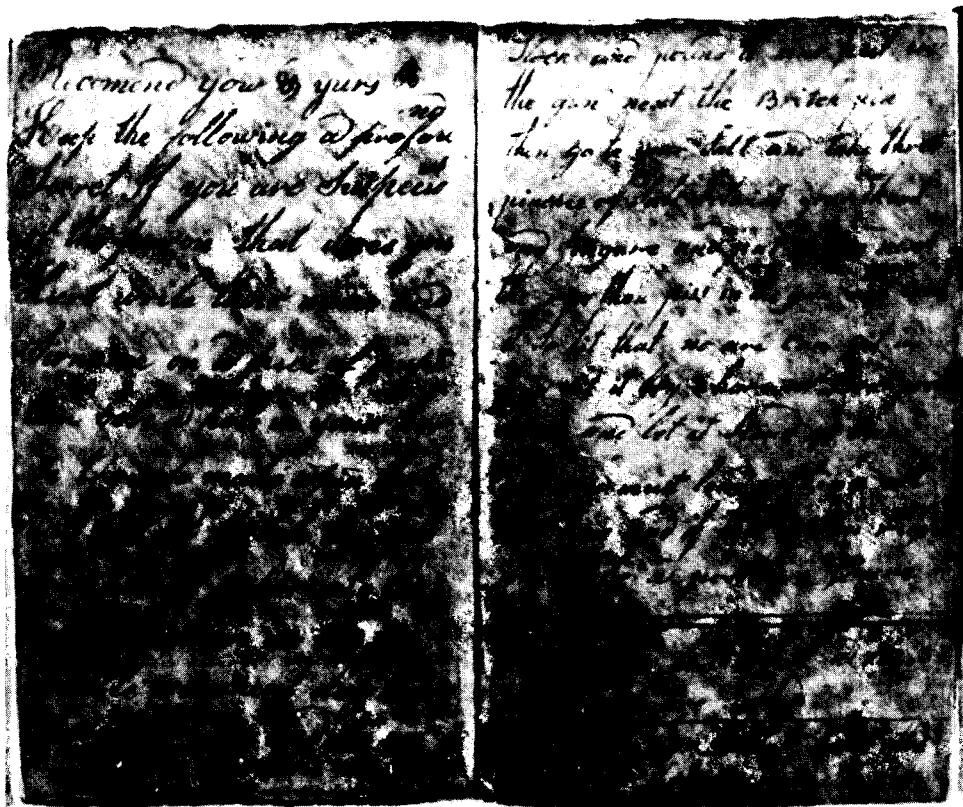


Figure 4: A "cure" for protection against "the Person who does you Hurt" from the only known surviving colonial compilation of occult recipes. Joshua Gordon "Witchcraft Book," 1784. Photograph courtesy of the South Caroliniana Library, University of South Carolina.

him Seized Maliciously [Abused]" appears seven times to fill the page. On the second page the phrase "Your Saviour Sweeting Blood [wch is yours]" is written nine times to fill the page. The phrases appear to link the pharisees' seizure of Christ to the Devil's seizure of persons through witchcraft and the ability of men and women to redeem themselves through Christ's blood. These same phrases, again copied to fill two pages, close the manuscript.⁴¹

Tucked between the ritual phrases or charms are fifteen pages of occult cures. Most of them are based on the traditional European notion of sympathetic attraction. In this formula, parts of bodies of sick or injured persons or animals are employed in rituals to solicit sympathetic healing in the same animal or person. Thus, Gordon's manuscript gives remedies using animal feces and human urine to cure "old soars" and rheumatism, to preserve cattle from witchcraft and other mysterious disorders, and to reveal those who

⁴¹ A Joshua Gordon filed for Revolutionary War compensation from the North Carolina government and another person by that name appeared as a resident of York County, South Carolina, in the 1790 federal census; information from Joshua Gordon file, South Caroliniana Library, University of South Carolina, courtesy of Allen Stokes, Director. The charms in the Gordon manuscript are difficult to decipher. Anita Rutman, a superb paleographer, suggests an alternate reading of the second charm to the one I have proposed here. She argues that the weight of evidence compels a transcription reading "Your Saviour Sweeting Blood with wry[?]" and comments that "'wry' can, according to the *OED*, be defined as 'a twisting or tortuous movement' which would make some sense and a fairly rhythmic little incantation." Rutman to the author, August 6, 1978.

intended harm. The remedies for warding off malevolent individuals follow English and early colonial tradition in assuming that these persons are neighbors in the local community. Thus, the compiler warns, “you must not lend any manner of thing off for the Space of 3 Days [from] your plantation. The person or persons guilty will assuredly come wanting to Borrow Something But by no means Lend; otherwise, you lay open a gain to thir malies [i.e., malice], which will be more desperate then Before.” One cure—“An Indian cure for the reipture [i.e., rupture] in children”—crossed cultural boundaries. Several cures linked occult rituals to Christianity. “A Cuar for gun that is Speld,” for example, required the frustrated hunter to “lod your gun in the name of the father, Son, and holy ghost and put the point of the tung [of a dead deer] down next the powder . . . and when you discharge your gun do it in the name of the father, son, and holy gost.” A cow losing milk could be cured if its owners would “take a heather belonging to a box Iron, put it in the fire, and make it Red hot [and then] take the milk of the cows thats hurt [and] power [i.e., pour] on the hot iron repeating the names of the blessed trinity.”⁴²

Still, most evidence suggests that occult practices only survived idiosyncratically in pre-Revolutionary America. The library lists in the estate inventories of eighteenth-century Virginia, including the largest libraries like that of Robert “King” Carter, contain almost no occult books.⁴³ Although basic astrological information still appeared in almanacs, after about 1720 no almanac-makers promoted sophisticated occult ideas as Pennsylvania’s Daniel Leeds and Jacob Taylor had done earlier. Rather, they developed a cynical ambivalence toward occult practices. Nathaniel Ames of Massachusetts, for example, vacillated frequently on the question of predicting the future. His warning to readers about wise men simultaneously supported and condemned their art. “Observe it you may,” Ames wrote in his 1752 almanac, “that Cunning Men are not always honest: trust them as you have tried them.”⁴⁴

A change in intellectual climate often reflects shifts in the sociology of belief more accurately than it does the complete disappearance of old ideas; typically, only certain social classes or groups, usually the educated elite, reject once-popular notions, while many others, usually the less educated, continue to accept them. The history of European and American witchcraft beliefs illustrates this phenomenon. In seventeenth-century Europe magistrates became increasingly reluctant to convict accused witches. That same reluctance finally became evident in America as well. No one was convicted of practicing witchcraft in Massachusetts after the Salem trials of 1692. In South Carolina a

⁴² These cures follow closely the style of many of the cures described by Keith Thomas; see his *Religion and the Decline of Magic*, 177–92.

⁴³ For the Carter Inventory, see the *William and Mary Quarterly*, 1st ser., 10 (1901–02): 232–41, and 11 (1902–03): 21–28. For book lists from other eighteenth-century estate inventories, see *ibid.*, 3 (1894–95): 132–33, 248–51, and 6 (1897–98): 158–64; and *Virginia Magazine of History and Biography*, 2 (1894): 225–26, 3 (1895–96): 388–91, and 4 (1896–97): 288–92.

⁴⁴ For a collection of the essays and poems in most of Ames’s almanacs, see Samuel Briggs, *The Essays, Humor, and Poems of Nathaniel Ames, Father and Son, of Dedham, Massachusetts . . .* (Cleveland, 1891). For the quotation, see Ames’s almanac of 1752 in *ibid.*, 236; also see *ibid.*, 80, 88, 120–22, 176–78, 187, 198–99, 226, 303.

year after Nicholas Trott urged grand juries to prosecute witches a witchcraft indictment was “returned Ignoramus.” The rebuff made a local Anglican clergyman, if not the accused, “stand amazed that the Spirit of the Devil should be so much respected as to make men call open Witchcraft Imagination and no more.”⁴⁵

Yet witchcraft beliefs continued to play important roles in other colonial settings. Colonists circumscribed their opinions, using witchcraft beliefs to manipulate local affairs while keeping them out of colonial courts. The experience of John Craig, a Scottish Presbyterian minister, in 1745 provides an example. After settling in Augusta County, Virginia, in 1740, Craig found the community divided by bitter quarrels of the kind that had distinguished witchcraft settings a century earlier. Craig’s status and occupation soon drew him into these quarrels. In the midst of the dispute, his wife became seriously ill during her first pregnancy. As a midwife delivered her child, Craig began to hallucinate. He dreamed that he hated his wife and the midwife, although he “knew not from what reason or cause.” Four months later the infant died. Finally, Craig’s cattle and horses died too. His livestock suffered from a disease contracted only by his cattle, although animals owned by neighbors that had been penned in the same quarters remained healthy.⁴⁶

Craig suspected witchcraft in the animal deaths. But he refused to make his opinion public, since he was convinced that the “Divel had higher Designe than to kill Brutes.” Nor did he blame witches for his wife’s sickness or his child’s death. Later, however, Craig apparently named the persons he believed responsible for poisoning his animals. Now his enemies accused Craig of resorting to occult practices. They said he “used Charms and named Neighbors as the instruments of my loss,” meaning that he had used occult techniques to secure the names of his transgressors. Yet neither Craig nor his antagonists pursued their accusations in the courts, even though to do so was still legally possible in Virginia. Instead, they allowed their belief in witchcraft to shape local affairs but kept them away from unsympathetic magistrates.⁴⁷

At the end of the colonial period, Yale’s Ezra Stiles assessed the occult practices that still existed in the colonies as only minor survivals of a more active occult era. “Something of it subsists among some Almanac makers and fortune tellers.” He found two of the latter in New England—a man in Tiverton, Massachusetts, who cast horoscopes and located lost objects for seamen, and a woman in Newport, Rhode Island, who made urine cakes for use in divining. But these practitioners illustrated the decline of earlier occult practices: “in general the System is broken up, the Vessel of Sorcery shipwreckt, and only some shattered planks and pieces disjoyned floating and

⁴⁵ Francis Le Jau to Philip Stubbs, April 15, 1707, in Frank J. Klingberg, ed., *The Carolina Chronicle of Dr. Francis Le Jau, 1706–1717* (Berkeley and Los Angeles, 1956), 25.

⁴⁶ “Autobiography of John Craig,” Montreat, N.C., Historical Foundation of the Presbyterian and Reformed Churches, MS.

⁴⁷ *Ibid.* Richard Dorson has described witchcraft episodes drawn from nineteenth-century New England local histories; some of them may have occurred in the eighteenth century but nearly all of them are undated. See Dorson, *Jonathan Draws the Long Bow* (Cambridge, 1946), 38–44.

scattered on the Ocean of . . . human Activity and Bustle.” Stiles’s judgment was accurate even in its metaphors, because part of the evidence pointing to the survival of astrology in the colonies comes from its popularity among seamen and ship captains who consulted practitioners like the man Stiles of Tiverton.⁴⁸

EXPLAINING WHY OCCULT RELIGIOUS PRACTICES declined in America is difficult. The explanations historians have given for Europe do not work well even there. Alan Macfarlane has traced the erosion of accusations of witchcraft in England to the triumph of the market economy. The new emphasis on monetary gain rather than village cohesion simply destroyed the neighborly intimacy in which witchcraft, and perhaps all of the occult arts, prospered. Keith Thomas has argued that the decline stemmed from the rise of more modern medicine and the expansion of technology. Yet historians disagree vociferously about the real impact of the market economy and the effectiveness of the new medicine. In addition, the technology Thomas discussed is limited largely to improvements in newspaper printing and distribution. Since the social significance of this technology is diminished by the continuing low rate of literacy in eighteenth-century England, it remains difficult to explain why occult religious practices disappeared there, especially among the large poor and illiterate populace, a criticism that also applies to the once-popular idea that a “scientific revolution” among English intellectuals erased occult ideas from England itself.⁴⁹ By reviewing the situation in America, however, we can delineate alternative causes that brought about the disappearance of occult religious practices in pre-Revolutionary America and, perhaps, Europe as well.

One important cause of the decline of occult religious practices was a change in the literary taste of England’s educated elite, leading to an increasing scarcity of occult reading materials in America. After 1700 works in Hermeticism, Rosicrucianism, alchemy, geomancy, and chiromancy simply were not published in England. The number of occult and astrologically based medical works declined drastically. The seventeenth-century authors of such works either changed their minds or died without leaving successors to continue their work. Colonists who hoped to sustain their occult convictions through reading had to collect out-of-print titles published in the previous century. Otherwise, they had little to peruse except Christian publications.⁵⁰

Personal discord and intellectual vacillation characterized what remained of the occult literary world in almanacs. Almanac-makers complained bitterly

⁴⁸ Franklin B. Dexter, ed., *Literary Diary of Ezra Stiles*, 3 vols. (New York, 1901), 1: 385–86; and George C. Mason, “The African Slave Trade in Colonial Times,” *American Historical Record*, 1 (1872): 311–19. For the first citation of both sources, see Kittredge, *The Old Farmer and His Almanac*, 39–41.

⁴⁹ Macfarlane, *Witchcraft in Tudor and Stuart England*, 200–07; Thomas, *Religion and the Decline of Magic*, 641–63; Monter, *Witchcraft in France and Switzerland*, 37–41; Midelfort, *Witch Hunting in Southwestern Germany*, 6–7, 121–63; and Hugh Trevor-Roper, *Religion, the Reformation, and Social Change* (London, 1967), 90–192.

⁵⁰ The 1650s was one of the most productive decades in the publication of occult books. See Thomas, *Religion and the Decline of Magic*, 226–27, 288.

that competitors stole their “ephemerides.” The career of Pennsylvania’s Jacob Taylor can be described in good part as a series of confrontations with rivals. These battles dated from early disputes with Daniel Leeds before 1710 to Taylor’s final disputes with William Ball and John Jerman between 1735 and 1745.⁵¹ In this setting, the drive for institutional organization characteristic of English Christian groups never flourished among astrologers and almanac-makers. An astrologers’ society formed in London in the 1650s died out in the 1680s. Only the vaguest hint points toward any organization of occult practitioners in the colonies. In his almanac for 1746 Jacob Taylor claimed that he learned astrology and the other occult practices he now detested from unnamed Pennsylvania “Sons of Art” in the late 1690s or early 1700s. Taylor, however, said nothing else about his occult education, and no additional evidence provides further information.⁵² Lacking both professional organization and personal discipline, almanac-makers responded weakly to the ridicule leveled at them by critics like Jonathan Swift and Benjamin Franklin. Increasingly, they declined to predict political events. After 1720 they seldom listed herbal signs for use in astrologically based medicine. Recipes for curing remedies emphasized natural physical properties in their ingredients rather than relationships to the planets.⁵³ These changes appear to parallel the deference to elite opinion that early American historians find pervasive in colonial politics. Dedham’s Nathaniel Ames summarized it bluntly in 1751. When astrology “was caressed by Princes and great Philosophers,” everyone defended it, but “now,” Ames wrote, “the Table is turned, they speak against it, and the multitude must follow.”⁵⁴

The multitude also was pushed. Former adepts like Jacob Taylor launched powerful assaults on occult religion when they abandoned occult practices themselves. Taylor linked astrology to the “filthy Superstition of Heathens” in an essay in his almanac for 1746 that denounced all occult practices. He charged that only four of twenty-seven astrologers he had known in Pennsylvania “could write plain English or spell common Words.” He argued that reputable physicians “never debase their Writings with Fables of Planets and Signs,” and he used alchemical imagery to deride the almanacs of rivals William Ball and John Jerman when he described their publications “as remote from Truth as Lead from Silver.”⁵⁵ Christian ministers used institutional authority to turn the laity away from occult practice. New England clergymen inveighed against sorcery and witchcraft for fifty years before the Salem affair. Quaker leaders in Pennsylvania quickly denounced the geomancy, necromancy, and astrology that surfaced during the Robert Roman affair of 1696.

⁵¹ Jacob Taylor, *An Almanack for . . . 1705* (Philadelphia, 1705), *Ephemeris Sideralus: An Almanack for . . . 1706* (Philadelphia, 1706), and *An Almanack and Ephemeris for . . . 1744* (Philadelphia [1744]); and Titian Leeds, *The American Almanack for . . . 1714* (New York, 1714).

⁵² Jacob Taylor, *An Almanack and Ephemeris for . . . 1746* (Philadelphia [1746]). The essay is scattered through the almanac. Leventhal, *In the Shadow of the Enlightenment*, 47, 263. Leventhal has accepted Taylor’s claim made in his 1746 almanac that he abandoned astrology as early as 1701. But a reading of Taylor’s almanacs in the 1710s and 1720s reveals an ambivalent attitude about the science.

⁵³ Leventhal, *In the Shadow of the Enlightenment*, 262–65.

⁵⁴ Quoted in Briggs, *The Essays, Humor, and Poems of Nathaniel Ames*, 226.

⁵⁵ Taylor, *An Almanack and Ephemeris for . . . 1746* (Philadelphia [1746]).

Sometimes ministers even had to warn colleagues about occult practices. In 1728 the Synod of Philadelphia received a complaint that the minister of the Presbyterian congregation in Freehold, New Jersey practiced judicial astrology. Although Joseph Morgan probably had only attempted to test the validity of the art through experimentation (he was something of an amateur scientist who once corresponded with London's Royal Society), the Synod could not dismiss the incident lightly. It emphatically denounced astrology even while dealing gently with Morgan and his troubled congregation.⁵⁶

Evangelical Christianity of the kind spread in the eighteenth-century colonial awakenings may have challenged occult religion directly. To be sure, no conversion of an adept in magic was recorded in the literature of the awakening. But the ritual forms of evangelical Christianity and occult religion paralleled each other in striking ways. In the English occult tradition, cunning persons, wise men, and astrologers manipulated ritual processes of doubt, inquiry, and resolutions to solve problems and gain clients. Clients came to them with perplexing problems. The wise man quizzed the client about his birth, personal habits, background, and recent disputes, then used geomancy, chiromancy, metoposcopy, horoscopes, or divination to effect cures for diseases, suggest the way to financial success, or locate the places where lost horses might be found.⁵⁷ Parallel processes characterized colonial Christian awakenings. As in meetings with cunning persons, settlers approached Christian evangelists with numerous fears, doubts, and problems. Awakening ministers reshaped these problems to suit Christian answers. They insisted that the settlers' real concern was salvation. Then, in simple sermons on elemental Christian doctrine, they began resolving those problems by explaining Christian faith. If listeners panicked when they discovered that their own salvation was in doubt, the panic increased the clergyman's authority, since he claimed that only the Christian God knew the future and controlled the world. Although we still do not quite know why American colonists endured this torment, most available evidence suggests that they thoroughly relished it. Colonists went to hear awakening evangelists like George Whitefield and with them enacted the ritual of doubt, inquiry, and resolution typical of those meetings again and again.⁵⁸

The Calvinism of the colonial awakenings also paralleled important occult ideas. The fatalism inherent in Calvinism's concept of predestination found an occult equivalent in the idea fundamental to astrology that motions of stars

⁵⁶ *Records of the Presbyterian Church in the United States of America, 1706–1788* (Philadelphia, 1904), 91; and Whitefield J. Bell, Jr., "The Reverend Mr. Joseph Morgan. An American Correspondent of the Royal Society, 1732–1739," *Proceedings of the American Philosophical Society*, 95 (1951): 254–61.

⁵⁷ Thomas, *Religion and the Decline of Magic*, 231–44; and Macfarlane, *Witchcraft in Tudor and Stuart England*, 211–53. One former adept who seems to have been affected by the awakenings was Pennsylvania's Jacob Taylor. His attacks on occult religion and propagation of Christianity in his almanacs became especially spirited after George Whitefield's first visit to Philadelphia in 1740.

⁵⁸ Despite the considerable attention lavished on the allegedly democratic character of the awakenings, in their religious aspects they are almost always studied from a ministerial rather than a lay perspective. For a contemporary layman's view of the dramatic significance of George Whitefield's activities, see Michael J. Crawford, ed., "The Spiritual Travels of Nathan Cole," *William and Mary Quarterly*, 3d ser., 33 (1976): 92–103.

and planets revealed a future that individuals could not control. Calvinist evangelists and occult practitioners also explained catastrophes in similar ways. Believers in occult ideas thought the coming of comets and eclipses had inescapable and usually disastrous consequences; not even kings and queens escaped their verdicts. No one escaped judgment by the Calvinist God either. Sometimes He damned seemingly model Christians simply to demonstrate His sovereignty.⁵⁹

Colonial governments also used coercion to steer colonists away from occult religion. The state used its authority to make orthodox Christianity the only legitimate religion in all the colonies. The colonies also banned occult practice and enforced all the laws on religion with enough frequency that colonists knew they assumed dangerous risks should they resort to occult worship. All of the English statutes outlawing occult practice applied in the colonies, including the famous statutes of James I that punished both white and black magic. But some colonial legislatures passed their own statutes in this area. As late as 1736 George Webb reminded Virginia magistrates that the colony's own laws obliged them to try settlers for practicing witchcraft, for using occult techniques to find lost goods, and for provoking "unlawful love." Maryland's seventeenth-century governors commonly demanded that grand jurors repress witchcraft, sorcery, and necromancy. Little wonder, then, that the colony's famous Act of Toleration of 1649 protected Puritans and Catholics from slander but gave no similar protection to sorcerers, wise men, or witches. The contrasts so frequently drawn between Massachusetts and Rhode Island in matters of religious tolerance disappear in a common approach to occult practice: both colonies demanded the execution of convicted witches.⁶⁰

Blasphemy statutes common to all of the colonies also repressed occult religion by making it illegal to slander by direct or indirect means Jesus and Christianity. They were important because colonial authorities viewed occult practice as a challenge to orthodox Christianity, even though some English men and women tried to synthesize them. Colonial legislators further penalized occult practice by permitting only Christians to hold public office. Seemingly tolerant Pennsylvania forced officeholders to swear to their belief in the divinity of Jesus, banned blasphemy, forbade Sunday labor, and urged all settlers to attend Christian services on the Sabbath so "looseness, irreligion, and Atheism may not creep in under pretense of conscience." Only seven colonies forced settlers to pay taxes to support specific churches—the Church of England in the southern colonies and New York and Congregational churches in New England. But all of the colonies ordered fines, jail, even

⁵⁹ For comment on this point, see Thomas, *Religion and the Decline of Magic*, 17, 21, 86–88, 332–34, 341–45, 370.

⁶⁰ George Webb, *The Office and Authority of a Justice of Peace* (Williamsburg, 1736), 61, 361–62; Arthur P. Scott, "History of the Criminal Law in Virginia during the Colonial Period" (Ph.D. dissertation, University of Chicago, 1919), 276–78; Anson Phelps Stokes, *Church and State in the United States*, 1 (New York, 1950): 191–92; *Archives of Maryland*, 72 vols. (Baltimore, 1883–), 52: liv–lv; F. N. Parke, "Witchcraft in Maryland," *Maryland Historical Magazine*, 31 (1936): 271–98; and John R. Bartlett, ed., *Records of the Colony of Rhode Island and Providence Plantations*, (Providence, 1856), 1: 166.

death for colonists who blasphemed Christ and engaged in occult activity, while most demanded some kind of public confession of Christian beliefs as the price for entering colonial politics.⁶¹

Did colonial officials enforce these statutes? Sometimes. They were especially vigilant against witchcraft. Some sixty witchcraft trials occurred in America before the 1692 Salem trials. If the ratio of executions to convictions is taken as a measure of intent to rid the New World of this occult art, then America was more dangerous for accused witches than England was. Alan Macfarlane and Keith Thomas have found that English magistrates were relatively lenient in cases of witchcraft. In Essex County, England the courts executed only about a fourth of those they convicted for practicing witchcraft after 1600, convicted about half those they tried, and executed nobody after 1645. In America the conviction rate was about the same: half of those tried were found guilty. But the courts executed fully half of the guilty, with nearly all executions coming *after* 1645. Thus, some twenty-two persons already had been executed for practicing witchcraft in America before the executioners at Salem hung and burned twenty more.⁶²

Colonial officials enforced the laws against less malevolent occult practices with greater irregularity. In his 1727 Boston almanac, Nathaniel Bowen complained that the colony's fortunetellers were not being prosecuted by officials. He was convinced that, "if the wholesome Laws of the Province were duly Executed on such Negro mancers, I would venture to Foretell what would soon be their Fortune."⁶³ Bowen's assertion is difficult to test—for reasons that affect all assessments of law enforcement practice. The knowledge of crime comes almost wholly from the very officials whose work is to be assessed. Therefore, there is little independent evidence to determine whether these officials actually restricted all known illegal behavior, very little of it, or did either consistently. Certainly, colonial court records seldom mention the prosecution of occult activity other than witchcraft; the cases in Chester County, Pennsylvania were notable exceptions to this pattern. Instead, if minor occult activity was repressed by the law, either as written or as practiced, such repression occurred indirectly when, for example, magistrates used the blasphemy statutes to cite colonists for swearing or enforced statutes forbidding labor on the Sabbath. These actions consistently warned colonists that magistrates intended to determine where blasphemous, heretical, and illegal religious activity started and where the orthodox practice of Christianity ended.⁶⁴

⁶¹ Stokes, *Church and State in the United States*, 168; Nicholas Trott, *The Laws of the British Plantations in America, Relating to the Church and to the Clergy* (London, 1721), 74-76, 211-15, 231-32, 242-43, 255-56, 268-69, 288-90, 297, 303, 324-26, 337, 345; and Carl Zollmann, *American Church Law* (St. Paul, 1933), 2-6.

⁶² Frederick C. Drake, "Witchcraft in the American Colonies, 1647-62," *American Quarterly*, 20 (1968): 694-725; and Macfarlane, *Witchcraft in Tudor and Stuart England*, 23-63.

⁶³ Nathaniel Bowen, *The New England Diary: or, Almanack for . . . 1727* (Boston, 1727).

⁶⁴ For a sampling of magistrates' action against anti-Christian behavior in Pennsylvania, see *Records of the Courts of Chester County Pennsylvania 1681-1697*, 141, 244, 393; *Records of the Courts of Chester County Pennsylvania* [2]: 22, 79, 145, 159; and *Records of the Courts of Quarter Sessions and Common Pleas of Bucks County Pennsylvania, 1684-1700* (Meadville, Pa., 1943), 20, 75, 76, 204, 340.

How the uneven prosecution of occult activity affected colonists can be determined in part through an analytical tool borrowed from sociology called labeling theory. Labelling theory, which is most commonly used to analyze police behavior, suggests that the identification of certain behavior as deviant is accomplished by many parts of the legal system, not just by trial verdicts. The prohibition of an activity in the written law and the mere apprehension of an individual for engaging in it, even when the individual might never be charged, tried, or convicted, often effectively sustains the categorization of that activity as illicit. Jack Douglas's description of the importance of arrest in modern America has special relevance to the colonial situation, where all of the agencies of state and church resolutely proclaimed an absolute need for settlers to obey the law: "Arrest by the police is the crucial stage in legal stigmatization in our society. It is arrest, not conviction, which makes an individual into a *distrusted outsider*." He continued, "When an individual is arrested by the police for anything, it creates a suspicion in the minds of most members of the society. How are they to check out the charge? Surely the police, who are seen as experts on such matters, know something about his activities that warrant such suspicion."⁶⁵

Two cases from Maryland and New York illustrate how the labelling process worked to repress occult religion in the colonies. These cases combine themes of government support for Christianity, efforts to control anti-Christian behavior, and official equation of magic with blasphemy. The Maryland case involved an otherwise obscure Jew named Jacob Lumbrozo, who was arrested for blasphemy in 1658. Lumbrozo ridiculed Christ's resurrection. He said that Christ's disciples "stole him away . . . by Negromancy or sorcery" and that Christ himself was a "Negromancer" who performed his miracles by "the Art Magick." In New York the offender was a relatively prominent individual. A Queens' justice of the peace named Jonathan Whitehead was cited in 1701 for equating Christian ministers with occult practitioners. Whitehead said that "Religion was onely an invention of cunning Men to gett thaire living by." Another justice, who reported Whitehead's statement to court officials, complained that Whitehead's attack on ministers as well as Whitehead's eagerness to travel on the Sabbath "tended to nothing less than Atheism and the discouragement of Christianity" and that "he being a Justice ought in a particular manner to take care not to give such examples."⁶⁶

Viewed from the traditional liberal perspective on American church-state relationships, the Lumbrozo and Whitehead cases point to the existence of religious freedom in the colonies, since neither man was ever tried for his

⁶⁵ Douglas, *American Social Order: Social Roles in a Pluralistic Society* (New York, 1971), 316. For a convenient explanation of labelling theory and a demonstration of the uses historians can make of it, see Eric H. Monkkonen, *The Dangerous Class: Crime and Poverty in Columbus, Ohio, 1860-1880* (Cambridge, 1975). For an extensive critique of labelling theory, see Walter R. Grove, ed., *The Labelling of Deviance: Evaluating a Perspective* (Beverly Hills, Calif., 1975); and, for an able defense, see Ken Plummer's review of Grove's *The Labelling of Deviance* in the *British Journal of Criminology*, 17 (1977): 79-81.

⁶⁶ *Proceedings of the Provincial Court of Maryland, 1658-1662*, in *Archives of Maryland*, 41: 203; Douglas Greenberg, *Crime and Law Enforcement in the Colony of New York, 1691-1776* (Ithaca, N.Y., 1976), 112; and E. B. O'Callaghan, ed., *Documentary History of the State of New York*, 4 vols. (Albany, 1849-51), 3: 200.

offense. But viewed through the lens of the labelling theorist, the cases illustrate the limitations of early American religious freedom. Maryland and New York officials feared the occult and acted to defend orthodox Christianity against it. By bringing Lumbrozo and Whitehead before the courts and entering their alleged comments in the permanent court record, colonial officials as much stamped the deviant label on “Art Magick” as on Lumbrozo and Whitehead. In this setting colonial officials did not need convictions to repress occult religion. No colonist could turn to it comfortably when the law made it illegal and officials arrested settlers for describing Christ as an occult master. Even trials in which falsely accused colonists were acquitted of practicing witchcraft, geomancy, and other occult arts helped repress occult religion. Perhaps these colonists were indeed innocent. No matter. To fellow colonists—and nearly all historians—their indictments and trials branded them forever as accused witches whose behavior contained at least some suspicious elements. In addition, the trials and the difficulties they produced again pointed to the danger of any association with occult practice. After all, these trials did not always result in acquittals. Especially in America they frequently produced convictions and executions.

UP TO THE EVE OF THE REVOLUTION, then, colonial law never recognized equality of religion in America. In a society where lawmaking remained the business of relatively affluent, church-connected citizens, New World legislation on religion reflected Old World tradition. The law became an instrument of state activity for the manipulation of religious allegiances. It limited legitimate religious expression to Christianity, most importantly by providing Christianity general support, least importantly by supporting specific Christian groups. It demanded the suppression of occult religion, whether or not occult practices were fused with or were used in place of Christian ones. In another time and setting the effects of these legal biases might have been minimal. But, when combined with intellectual decay in the English occult tradition, discord among partisans of occult practices, and failure of those partisans to create institutions to defend themselves and their arts, the chill of the law became an important cause in the destruction of occult means of invoking superhuman powers and beings in human affairs that had survived in the English colonization of America.

The decline of the English occult tradition in the colonies should not, however, be taken as proof that occult practices were no more than fascinatingly aberrant features of America’s religious heritage. Rather, their earlier presence in the colonies should lure us into exploring a broad range of non-Christian and quasi-Christian religious practices, the history of which in America remains almost wholly unwritten. Despite their general decline in the colonies, at least a few survived into the nineteenth century. This was espe-

cially true in the rural South and in German-speaking portions of Pennsylvania. The emphasis on healing that Cotton Mather believed accounted for the colonial wise man's success remained strong as America entered the industrial age. Some Christian groups promoted healing anew, while patent medicines, healing therapies, and pseudo-sciences like phrenology gave it secular expression. The occult even may have been reinforced by early scientific medicine, whose claim to success usually outdistanced achievement. The law also continued to penalize occult practices. Here a study of modern spiritualism is especially relevant. Although religious historians have usually argued that coercion cannot destroy religious belief, an anthropologist, Irving Zaretsky, has disputed this claim. His study of San Francisco spiritualist churches in the 1960s demonstrates that simple fear of arrest among spiritualist ministers shaped their religious language and basic beliefs, even though actual police harassment seldom occurred. Elsewhere, the relationship between occult practice and Christian belief among laypersons may have been creative rather than hostile, despite opposition from mainstream denominational leaders. The formal Spiritualist movement of the antebellum North had strong roots in the Unitarian movement. In the nineteenth-century South, evangelicalism and occultism flourished side by side among both whites and blacks.

In this regard an observation by Thomas Wentworth Higginson, a nineteenth-century Unitarian turned Spiritualist, is especially apt. Writing on the eve of the Civil War, Higginson argued that lay religious opinion spilled far beyond the perimeters of the familiar denominational creeds. "When the minister of a cold, conservative church preaches his last closing climax of sermons against spiritualism, he little knows that of the church membership who sit patiently beneath him, more than one half are spiritualists already in their hearts."⁶⁷ In his time Higginson meant to ridicule the shallowness of religious commitment and behavior in mainstream Christian groups. In our time, despite exaggeration, his words should act as a formidable challenge to explore all facets of an American religious heritage that extends far beyond the great denominations, churches, ministers, and a few prestigious laypersons.

⁶⁷ Thomas W. Higginson, *The Results of Spiritualism* (New York [1859]), 19. For useful introductions to occult religion in nineteenth- and twentieth-century America and its relationship to Christianity, see R. Laurence Moore, "Spiritualism," in Edwin S. Gaustad, ed., *The Rise of Adventism: Religion and Society in Mid-Nineteenth-Century America* (New York, 1974), 79–103, and *In Search of White Crows: Spiritualism, Parapsychology, and American Culture* (New York, 1977); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 215–24, 255–56; Tom Peete Cross, "Witchcraft in North Carolina," *Studies in Philology*, 16 (1918–19): 217–87; Loudell F. Snow, "The Religious Component in Southern Folk Medicine," *The Conch: A Sociological Journal of African Culture and Literature*, 8 (1976): 26–51; David E. Harrell, *All Things are Possible: The Healing and Charismatic Revivals in Modern America* (Bloomington, Ind., 1975); and Irving I. Zaretsky, "In the Beginning Was the Word: The Relationship of Language to Social Organization in Spiritualist Churches," in Irving I. Zaretsky and Mark P. Leone, eds., *Religious Movements in Contemporary America* (Princeton, 1974), 166–219.

The Rise of Stalin's Personality Cult

ROBERT C. TUCKER

THE CULT OF LENIN, which Lenin himself opposed and managed to keep in check until incapacitated by a stroke in March 1923, subsequently became a pervasive part of Soviet public life. No single cause explains its rise. Undoubtedly, the Bolsheviks genuinely venerated their *vozhd'* as the man whose personal leadership had been critically important for the movement from its origin to its assumption of power and for the creation and consolidation of the Soviet regime in the ensuing years. But it is also true that after Lenin's death that regime had a pragmatic need for a prestigious unifying symbol. The Lenin cult, whose obvious religious overtones were at variance with the Communist Party's professed secularism, is likewise an example of how Soviet culture came to incorporate certain elements of the Russian past, in this case the ruler cult. For centuries the Russian people, overwhelmingly composed of peasants, had been monarchist in outlook. The Revolution had opened the door for many peasant sons to have careers in the new society. Industrialization and collectivization resulted in the recruitment of millions of people of peasant stock into the working class. They brought with them, along with their Soviet schooling and experience, residues of the traditional peasant mentality, including respect for personal authority, whether it emanated from the immediate boss or from the head of the party and state. The social condition of Russia at the time of the "great turn" (1929-33) was, therefore, receptive to the cult of a deceased leader—or a living one.

Lenin refused to tolerate public adulation—save, with extreme reluctance, on his fiftieth birthday in 1920—and even then he showed dry disapproval of the eulogizing to which his comrades subjected him. Thus, as the public adulation of a living leader, the Stalin cult deviated from previous Bolshevik practice. How and why, then, did the Stalin cult arise?

Realpolitik FUSED WITH PSYCHOLOGICAL NEEDS. Politically, a Stalin cult alongside of and integrated into the Lenin cult promised to make Stalin's position

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more impregnable than it was at the start of the 1930s. Although he had won considerable support and even popularity inside party circles during the early post-Lenin years, Stalin never enjoyed a prestige even remotely comparable to Lenin's. His popularity, moreover, plummeted in the early 1930s as a result of forced collectivization and the concomitant famine of 1932–33. No evidence suggests that he was then in danger of being overthrown; still, his power was not yet absolute, the argumentative-critical tradition lived on (at least in higher party circles), and he had no guarantee against the rise of new opposition in response to new tribulation. So Stalin was undoubtedly concerned to forestall future trouble by making his political supremacy more unassailable. He was shrewd enough to realize that his elevation to a Lenin-like eminence in the regime's publicity would be useful for this purpose. But, important as it was, the political motive does not provide a sufficient explanation. Not only did the cult continue to grow after Stalin's power became increasingly absolute later in the 1930s, but both direct and indirect evidence indicates that it was a prop for his psyche as well as for his power. Boundlessly ambitious, yet inwardly insecure, he had an imperative need for the hero worship that Lenin found repugnant.

That the name "Stalin" symbolized a lofty idealized self to its seemingly earthy bearer was not widely known in Russia. In part, this reflected Stalin's studied effort to emulate in public Lenin's example of modestly unassuming deportment. In private, moreover, Stalin repeatedly affected disdain for adulation. For example, he concluded a letter to an Old Bolshevik, Ia. M. Shatunovskii, in August 1930 by saying, "You speak of your 'devotion' to me. Perhaps that phrase slipped out accidentally. Perhaps. But if it isn't an accidental phrase, I'd advise you to thrust aside the 'principle' of devotion to persons. It isn't the Bolshevik way. Have devotion to the working class, its party, its state. That's needed and good. But don't mix it with devotion to persons, that empty and needless bauble of intellectuals."¹

But the man behind the mask of modesty was hungry for the devotion he professed to scorn. He showed it by his own actions and by those of functionaries representing him—and by his acceptance of the officially inspired adulation as it rose in intensity during the 1930s. Indeed, in the very month in which he wrote the letter to Shatunovskii, Stalin, also in private, gave lie to that same advice. In June-July 1930 the Sixteenth Party Congress witnessed an outpouring of public tributes to him. Louis Fischer, who covered that event for *The Nation*, concluded his post-Congress dispatch by saying,

A good friend might also advise Stalin to put a stop to the orgy of personal glorification of Stalin which has been permitted to sweep the country. . . . Daily, hundreds of telegrams pour in on him brimming over with Oriental super-compliments: "Thou art the greatest leader . . . , the most devoted disciple of Lenin," and the like. Three cities, innumerable villages, collectives, schools, factories, and institutions have been named after him, and now somebody has started a movement to christen the Turksib

¹ I. V. Stalin, *Sochineniia*, 13 vols. (Moscow, 1946–52), 13: 19. The letter was first published in Stalin's collected works after the Second World War.

the "Stalin Railway." I have gone back over the newspapers from 1919 to 1922: Lenin never permitted such antics and he was more popular than Stalin can ever hope to be. It exposes a weak side of Stalin's character which his enemies, who are numerous, are sure to exploit, for it is as un-Bolshevik as it is politically unwise. If Stalin is not responsible for this performance he at least tolerates it. He could stop it by pressing a button.²

A press section officer of the Foreign Commissariat, whose duties included the briefing of Stalin on foreign press coverage of Soviet affairs, later confided to Fischer that, when he translated the passage just quoted, Stalin responded with an expletive: "the bastard!" (*svoloch*!).³ Evidently, he was stung by the truth of Fischer's observation that he himself bore responsibility for the emerging Stalin cult.

PRECISELY WHEN THIS CULT took on a life and momentum of its own is not easy to pinpoint. If the official celebration of Stalin's fiftieth birthday in 1929 is taken as the opening episode, there is no immediate sequel. The marking of Lenin's fiftieth birthday had been a one-time affair, and many in high positions may have assumed that Stalin's fiftieth would be similarly observed. Six months later came the acclaim at the Sixteenth Congress. But again the wave subsided. Although his name appeared often in the Soviet press, no steady stream of Stalin idolatry appeared in Soviet publicity in 1930 and most of 1931. Shortly afterwards, however, the cult began to grow. And Stalin himself took certain steps to make it happen.

One such step was in philosophy, one of the numerous fields in which different schools of thought contended for primacy in the relatively pluralistic atmosphere of the period of the New Economic Policy (NEP). In the mid-1920s the so-called mechanistic materialists lost their previously influential position, and a school of devotees of Hegelian dialectics, led by A. M. Deborin, won dominance. Theirs was a positive response to Lenin's invitation to Soviet philosophers in 1922 to constitute themselves a society of "materialist friends of Hegelian dialectics."

Although Lenin had some philosophical writings to his credit, it was not uncommon in the 1920s to place him below Georgii Plekhanov as a Marxist philosopher. Deborin's disciples, moreover, tended to rate Deborin as the Engels of his own time in the field of philosophy.⁴ Stalin, by contrast, was widely regarded in Communist Party circles as a *praktik*, save for his theoretical work on the nationalities problem and his codification of Leninist doctrine in *The Foundations of Leninism*; thus, his standing in Marxist philosophy was virtually nil. Interesting evidence on this point exists in the form of a list, published in 1929, of writings with which students entering graduate work in the Communist Academy's Institute of Philosophy were supposed to be

² *The Nation*, August 13, 1939, p. 176.

³ Louis Fischer gave me this information in a personal conversation in 1965.

⁴ David Jo:avsky, *Soviet Marxism and Natural Science, 1917-1932* (New York, 1961), 170.

familiar in advance. Thirty-three works were listed under dialectical and historical materialism—that is, philosophy. Six works by Marx and Engels came first, followed by six works by Lenin, then four by Plekhanov, and then seven by Deborin. Then came entry number 23, Stalin's *Problems of Leninism*, which even at that low ranking was very probably included for diplomatic reasons. The list ended (Western philosophers will be interested to note) with Descartes, Hobbes, Hume, and Berkeley.⁵

For both political and personal reasons, Stalin could not be content with this situation. As the party's *vozhd'* in succession to Lenin, he was duty-bound, in terms of Bolshevik culture, to be a creative Marxist theoretical mind of the first rank—in the political if not in the technical philosophical sense. But beyond those political expectations imposed by the *vozhd'*-role, Stalin had a personal craving for renown as a Marxist theoretician. Nikolai Bukharin, who knew him well, saw this and stressed it in his clandestine conversation with Lev Kamenev in 1928. For many years Stalin had harbored pretensions in Marxist philosophy. He had set forth what he saw as the fundamentals of dialectical materialism in his treatise of 1906–07, *Anarchism or Socialism*.² In correspondence in 1908 that vexed Lenin, Stalin had characterized Lenin's philosophical polemics with the Bogdanov group over Machism as a “tempest in a teacup” and commended A. A. Bogdanov for pointing out some “*individual* faults of Ilyich.”⁶

Stalin quietly continued, in the midst of intense political activities of later years, to try to enhance his command of Marxism as philosophy. He called upon Jan Sten, a leading philosopher of the Deborin school, to guide him in the study of Hegelian dialectics. Sten's teaching method, the one then used in the Institute of Red Professors, involved the parallel study of Marx's *Capital* and Hegel's *The Phenomenology of the Mind*. Stalin continued to have twice-weekly sessions with Sten from 1925 until some time in 1928, after which Stalin called a halt. Sten reportedly was depressed by the difficulty Stalin had in mastering Hegelian dialectics.⁷

Stalin sounded the characteristic note of the future Stalin school when he told a conference of agrarian Marxists on December 27, 1929 that Marxist theory always needed to keep in step with current practice. Not long afterwards, two young, clever, opportunist-minded philosophers from the Institute of Red Professors, Pavel F. Iudin and Mark B. Mitin, took up the same theme. Along with a third professor, V. Ral'tsevich, they published in *Pravda* on June 7, 1930 a long article that championed the notion that philosophy should apply itself in a new way to the theoretical problems of practice in building socialism. They lauded Stalin for showing an example of “deepened under-

⁵ *Vestnik kommunisticheskoi akademii*, 1929, Kn. 35–36, p. 390. For note of this list, see Joravsky, *Soviet Marxism and Natural Science*, 227.

⁶ I. Dubinskii-Mukhadze, *Ordzhonikidze* (Moscow, 1963), 93. For Bukharin's comment, see the Bukharin-Kamenev Conversations of July 11–12, 1928, Harvard University, Cambridge, Mass., Trotsky Archives, T 1897.

⁷ Roy A. Medvedev, *K sudu istorii: Genezis i posledstviia Stalinizma* (New York, 1974), 433. The information on the Stalin-Sten sessions came to Roy Medvedev from Sten's friend, E. P. Frolov.

standing of Marxist-Leninist dialectics” in his theoretical formulation of the idea of a struggle on two fronts—that is, against deviations of both Left and Right—and called for a corresponding philosophical struggle on two fronts. Although the authors did not openly attack Deborin, the article pointed to his school as the enemy on the philosophical second front. The authors came forward, in effect, as the nucleus of a new, Stalin school in Soviet philosophy. Stalin’s approbation—if not inspiration as well—was reflected in the unusual note, published along with the article, that claimed that “the editors associate[d] themselves with the main propositions of the present article.”

Soon Stalin personally intervened on the philosophical front. On December 9, 1930 he spoke out on philosophical matters in an interview with a group of philosophers from the Institute of Red Professors. Mitin later quoted him as saying that it was necessary to “rake and dig up all of the manure that has accumulated in questions of philosophy and natural science.” In particular, it was necessary to “rake up everything written by the Deborinite group—all that is erroneous on the philosophical sector.” Deborin’s school was a philosophical form of revisionism that according to Stalin, who had a special talent for coining caustic neologisms, could be called “Menshevizing idealism.” It was necessary, he continued, to expose a number of erroneous philosophical positions of Plekhanov, who had always looked down upon Lenin. Stalin kept emphasizing in the interview that Lenin had raised dialectical materialism to a new plane. Before Lenin, he said, materialism had been atomistic. On the basis of new scientific advances, Lenin produced a Marxist analysis of the electronic theory of matter. But, although he created much that was new in all spheres of Marxism, Lenin was very modest and did not like to talk about his contributions. It was incumbent upon his disciples, however, to clarify all aspects of his innovative role.⁸

Stalin was assuming the role of the premier living Marxist philosopher. Albeit coarsely, he spoke as one philosopher, and the authoritative one, to other philosophers. He was clearing the way for self-elevation by mobilizing the subservient, young, would-be disciples to dethrone Deborin and Plekhanov from their positions of eminence in the minds of Soviet Marxist philosophers. “Deborinism” along with “Menshevizing idealism” now became polemical by-words for philosophical heresy in the philosophical journal, *Under the Banner of Marxism*, and other publications. Future lists of mandatory advance reading for graduate students in philosophy no longer put Stalin in twenty-third place, and Deborin’s learned treatises did not figure in them at all.

In the interview Stalin did not directly refer to his own philosophical credentials, although he implied them by his pronouncements. But he employed an indirect strategy of cult-building by the way in which he dealt with Lenin. Since he did not actually harbor much enthusiasm for Lenin’s philosophical merits, why did he studiously praise Lenin as a philosopher and

⁸ Mark B. Mitin, *Boevye voprosy materialisticheskoi dialektiki* (Moscow, 1936), 43–44, and “Nekotorye itogi i zadachi raboty na filosofskom fronte,” *Pod znamenem Marksizma*, 1 (1936): 25–26. For the date of the interview, see the chronology in Stalin, *Sochinenia*, 13: 401. The full text of his remarks to the philosophers remains unpublished.

warn the audience not to be put off by Lenin's modest forbearance to speak about his contributions in this field? For one thing, there was the subtle Aesopian message, which could not have escaped the minds of the alert Iudin and Mitin, that *they* should not be put off by Stalin's own modesty on the same count. But, more importantly, Stalin was promoting Lenin's primacy in philosophy as a vehicle for his own claim to similar primacy. The party's erstwhile politico-ideological chief was presented as its philosophical chief as well—in place of Plekhanov, the acknowledged father of Russian Marxism, who had later become a Menshevik. By thus putting supreme philosophical authority into Lenin's *vozhd'*-role, Stalin helped the philosophers to grasp this broadened conception of that role as applicable to Lenin's successor.

They were quick to do so. In 1931 the organ of the Central Committee, *Bolshevik*, carried a bitter criticism of "Menshevizing idealism" as found in the *Great Soviet Encyclopedia*. Deborin's *Encyclopedia* article on Hegel was the first object of attack. In castigating Deborin and others of his school as carriers of Menshevizing idealism, the *Bolshevik* author stated, "Materialist dialectics really must be elaborated. But this elaboration must be carried out on the basis of the works of Marx, Engels, Lenin, and Stalin. . . ."⁹ Here appeared the holy quartet—Marx, Engels, Lenin, Stalin—who together became the symbolic centerpiece of Stalinist thought and culture, replete with the four huge, equal-sized portraits on the facade of Moscow's Bolshoi Theater for May Day, November 7, and other special occasions.

The cult of Stalin as Communism's first philosopher in succession to Marx, Engels, and Lenin had now been founded. But this was not all. Embryonic in this development was the monolithism that became a hallmark of Stalinist intellectual culture in all fields and that distinguished it from pre-Stalinist Bolshevism. To treat, for example, Lenin's philosophical writings, much less Stalin's, as sacrosanct dogma had never before been mandatory.¹⁰ Stalin himself became not only the first philosopher but also the authority figure in some other fields, and in still others a Stalin-surrogate—Andrei Vyshinskii, for example, in jurisprudence—was, so to speak, subenthroned as the authority figure. Part of the role of such Stalin-surrogates was to glorify Stalin's thought in the process of hunting for heresy and establishing Stalinist truth for their own disciplines. Consequently, those chosen as Stalin-surrogates were scholars who combined intellectual acumen, in most cases, with absolutely reliable servility. Anyone with any independence of mind, no matter how zealous a servitor of Communism, was unacceptable.

If Marxist philosophy was the first area Stalin selected for building the stately edifice of the Stalin cult, party history was the second. Here he moved into a field of great political sensitivity, for the annals of the Bolshevik past were the movement's inner sanctum. But he also trod on ground of intense

⁹ P. Cheremnykh, "Men'shevistvuiushchii idealizm v rabotakh BSE," *Bol'shevik*, no. 17, September 15, 1931, p. 85.

¹⁰ For a discussion by a former Soviet economist of this aspect of Stalinism and the use of "monolithism" to describe it, see Aron Katsenelinboigen, "Conflicting Trends in Soviet Economics in the Post-Stalin Era," *Russian Review*, October 1976, pp. 374–76.

personal concern, namely his own revolutionary biography. Nothing was of more importance to a man who felt driven to view himself as Bolshevism's second Lenin, in the past as well as the present. He made his move in the familiar manner that so many have chosen in their effort to set the record straight: he wrote a letter to the editors.

AT THE OUTSET OF THE 1930s, research on the history of the Marxist movement was still pursued with a certain freedom, contentious issues were seriously debated, and work of genuine scholarly character was still produced in Soviet Russia. One set of questions, those concerning the German Social Democratic Party (SPD) and the pre-1914 Second International, was deemed of sufficient interest that in 1929 the Communist Academy's Institute of History established a special group to study them; the group's academic secretary was A. G. Slutskii. Various articles by members of the group were published, one of which appeared in the journal *Proletarian Revolution* in 1930. Slutskii's main topic was Lenin's position in connection with the internal divisions in the pre-1914 SPD. The revisionist wing of that party, led by Eduard Bernstein, was opposed by a dominant centrist group, whose leaders were Karl Kautsky and August Bebel and whose viewpoint was taken by many—Lenin included—to be genuine revolutionary Marxism. On the extreme Left was a group of radicals led by Rosa Luxemburg. Slutskii claimed that as early as 1911 she had grasped and openly discussed the basically "opportunist" nature of Kautskyan centrism, whereas Lenin, though he had shown a certain critical caution toward the Kautsky-Bebel leadership ever since 1907, had continued to base his hopes on it. Lenin himself admitted in a letter of October 1914 that "Rosa Luxemburg was right"; he had not seen through Kautsky's pseudo-revolutionism as early as had the German left radicals. Slutskii concluded that Lenin had displayed "a certain underestimation of the centrist danger in the German party before the war."¹¹

The publication of this article demonstrates that, although a Soviet Lenin cult existed in the early 1930s, it was still possible to publish an article that did not treat Lenin as an icon—infalible, preternaturally foresightful, beyond human limitations. True, the editors of *Proletarian Revolution*—the Old Bolsheviks M. Saveliev, V. V. Adoratskii, M. S. Ol'minskii, D. Baevskii, and P. Gorin—seemed to sense the potential danger, for they inserted an introductory footnote disclaiming any agreement with Slutskii's interpretation of Lenin and announcing the printing of his essay "for purposes of discussion" only. But they clearly were unprepared for the thunderbolt that its appearance provoked from on high. Stalin was infuriated. He wrote a letter of article length, entitled "On Some Questions of the History of Bolshevism," which was simultaneously printed in *Proletarian Revolution* and *Bolshevik* at the end of October 1931.

¹¹ A. Slutskii, "Bol'sheviki o germanskoi s.-d. v period ee predvoennogo krizisa," *Proletarskaia revoliutsiia*, 6 (1930): 37-72.

First, Stalin mauled Slutskii's position beyond recognition, contending that to accuse Lenin of underestimating the danger of "veiled opportunism" was to accuse him of not having been a "real Bolshevik" before 1914: a real Bolshevik could never underestimate the danger of veiled opportunism. It was simply axiomatic that Bolshevism arose and grew strong in its ruthless struggle against all shades of centrism. Thus, the editors should never have accepted Slutskii's "balderdash" and "crooked pettifogging" even as a piece for discussion; the genuineness of Lenin's Bolshevism was not discussable. Second, Stalin protested Slutskii's favorable treatment of Rosa Luxemburg and the left radicals in the pre-1914 SPD. He was profoundly irked by the very idea that Lenin might have had something to learn from these people.

The strong Russian-nationalist tinge of Stalin's Bolshevism was also evident in his letter. He presented a Russocentric view of the history of the European Marxist movement: "Russian Bolsheviks" had a right to treat their own positions as the test of the Marxist revolutionary validity of those of left Social Democrats abroad. Lenin's forecast of 1902 in *What Is To Be Done?*—that the Russian proletariat might yet become "the vanguard of the international revolutionary proletariat"—had been brilliantly confirmed by subsequent events. "But does it not follow from this that the Russian Revolution was (and remains) the key point of the world revolution, that the fundamental questions of the Russian Revolution were at the same time (as they are now) the fundamental questions of the world revolution? Is it not clear that only on these basic questions could one really test the revolutionism of the left Social Democrats in the West?" Neither before nor after the war were Western Marxists to give lessons to their Russian brethren, but vice versa.

To say or imply otherwise, as Slutskii did, was "Trotskyist contraband." To give weight to this ugly charge, Stalin asserted that Slutskii's thesis about Lenin's pre-1914 underestimation of centrism was a cunning way of suggesting to the "unsophisticated reader" that Lenin had only become a real revolutionary after the war started and after he had "re-armed" himself with the help of Trotsky's theory that bourgeois-democratic revolutions grow into socialist ones (the theory of permanent revolution); Lenin himself, Stalin recalled, had written in 1905 that "we stand for uninterrupted revolution" and "we will not stop half way." But "contrabandists" like Slutskii were not interested in such facts, which were verifiable from Lenin's writings. Slutskii, Stalin noted elsewhere in the letter, had spoken in his article of the unavailability of some Lenin documents pertaining to the period in question. "But who except hopeless bureaucrats can rely on paper documents alone? Who but archive rats fail to realize that parties and leaders must be tested by their *deeds* primarily and not simply by their declarations?"

Toward the end of the letter, Stalin's language shifted from the rude to the sinister. In giving Slutskii a forum for his contraband, the editors were guilty of that "rotten liberalism" toward Trotskyist tendencies that was current among a segment of Bolsheviks who failed to understand that Trotskyism had long since ceased to be a faction of Communism but had turned into a forward

detachment of the counterrevolutionary bourgeoisie, making war on Communism, the Soviet regime, and the building of socialism in the USSR. Such, for example, was the purpose of the Trotskyist theses on the impossibility of building socialism in Russia and the inevitability of Bolshevism's degeneration.

Here Stalin repeated in public the argument of a memorandum he had written in 1929.¹² Its purport had been to transfer Trotskyist affiliation or sympathies from the category of political error to that of crime against the Soviet state and, hence, to justify repressive action against persons accused of being Trotskyist. As Stalin now spelled out the conclusion to his argument, "Liberalism toward Trotskyism, even though defeated and masked, is thus a form of bungling that borders on crime, treason to the working class." Hence, the editors' task, Stalin continued (mixing his metaphors), was "to put the study of party history onto scientific Bolshevik rails and to sharpen vigilance against Trotskyist and all other falsifiers of the history of our party, systematically ripping off their masks." This task was all the more necessary in that certain genuinely Bolshevik party historians were themselves guilty of errors that poured water on the mills of the Slutskiis. Unfortunately, said Stalin at the end, one such person was Comrade Emelian Iaroslavskii (the dean of Bolshevik party historians as well as the secretary of the Central Party Control Commission), whose books on party history, in spite of their merits, contained a number of errors in principle and of historical character.¹³

Considering what Stalin had said earlier about centrism, it is easy to see why he was outraged by Slutskii's argument that Lenin had underestimated the centrist danger in the German Social Democratic Party. To fight against deviations of the Left and Right was not to be a centrist, Stalin had contended in 1928, any more than it had been centrist of Lenin to combat both Menshevism on the Right and the sectarianism condemned in *Left-Wing Communism* on the Left. Centrism meant "adaptation" and on that account was "alien and repulsive to Leninism."¹⁴ How then—no matter what documents the archive rats might turn up—could a real revolutionary (that is, a Bolshevik), ever, even briefly, underestimate the centrist danger? To a mind that so reasoned, people like Slutskii fully deserved the merciless bawling out that the letter gave them and severe punishment as well. Slutskii was arrested in the later Stalin terror and spent many years in a concentration camp.¹⁵

But Stalin's letter, in addition to expressing his rage, pursued a tripartite purpose in cult-building. Though it did not mention his own name (how could it?), the letter solicited a Stalin cult in party history just because Stalin wrote it and by the tone and content. First, in writing it (or, conceivably, having it written to his specifications and issued in his name), he arrogated to

¹² Stalin, "Dokazitel'stvo," in *Sochineniia*, 11: 313–17. This document has the appearance of an internal Politburo memorandum.

¹³ Stalin, *Sochineniia*, 13: 84–102.

¹⁴ Stalin, *Sochineniia*, 11: 281–82, 284.

¹⁵ I am indebted to Roy A. Medvedev and Stephen F. Cohen for the information on Slutskii's subsequent arrest and imprisonment.

himself the position of premier party historian and arbiter of contentious issues in that sensitive area. For this the letter did not have to mention Stalin's name, but only to be the thoroughly dogmatic document that it was and to bear his signature. Merely by publishing the letter Stalin asserted his place as the supreme authority on the very subject that formed the core of the personality cult as it mushroomed in the 1930s: Bolshevism's past and the parts that he and others had played in it.¹⁶

Second, in the letter just as in the earlier interview with the Mitin-Iudin group of philosophers, Stalin followed the strategy of cult-building via the assertion of Lenin's infallibility. By making the party's previous *vozhd'* an iconographic figure, beyond limitation and beyond criticism, Stalin's letter implicitly nominated the successor-*vozhd'* for similar treatment. Since Stalin was the man whom the party had saluted in 1929 as its acknowledged chief in succession to Lenin, it behooved party historians to be as careful not to find lapses or blemishes in his political past as the letter in effect ordered scholars to be where Lenin's past was concerned. People as experienced in reading delphic utterances as were Bolshevik party intellectuals were bound to draw this inference as they pondered or discussed with one another the implications of the letter. Stalin even gave them a broad hint with a phrase used twice in the letter: "Lenin (the Bolsheviks)." Lenin, by Stalin's fiat, stood for true Bolshevik revolutionism as distinct from any and all false varieties—left, right, or center. The words in parentheses pluralized his revolutionary rectitude; they made it more inclusive without giving names. But anyone with intelligence enough to be a party historian could guess whose name ought to come next on the list of "Bolsheviks" in Stalin's normative sense of the term.

Third, the letter demanded quite explicitly that the party pasts of real revolutionaries be evaluated not on the basis of documents that archive rats might turn up or fail to uncover but on the basis of their "deeds." Naturally, such deeds would have to be documented insofar as possible. Stalin was to become the arch-archive rat of the Soviet Union or, more precisely, the leader of a whole pack, although he often hungered as much for the destruction or concealment of documents as for their discovery or publication. To those capable of discerning his letter's implications, they were that a party historian should not be guided, as had Slutskii, by what he could document, but by what he knew *a priori* must be true—that Lenin, being a "real Bolshevik," could never have underestimated centrism or that Stalin, also a "real Bolshevik," could never have taken an un-Bolshevik position at any juncture. The function of documentary materials, or of their concealment, was to help establish such higher truths. To use them otherwise was to slander and to

¹⁶ On the effect of the letter's rude style and tone, see, for example, V. A. Dunaevskii, "Bol'sheviki i germanskii levye na mezhdunarodnoi arene," in *Evropa v novoe i noveishee vremia: Sbornik statei pamiati Akademika N. M. Lukina* (Moscow, 1966). A modern Soviet historian, Dunaevskii has claimed that "the form of Stalin's pronouncement—sharp expressions against the authors he mentioned and politically characterizing them as 'rotten liberals,' 'Trotskyist contrabandists,' and the like—led to the impossibility of creative discussions on matters of principle and subsequently to repressions against individuals whom he had subjected to criticism"; *ibid.*, 508.

falsify. Consequently, the message of Stalin's tirade against falsifiers was that scholars had to be ready to falsify (in the normal meaning of the word) whenever *a priori* party-historical truth—as revealed by word from Stalin or his spokesmen—should so dictate.

The cult-building purport of Stalin's letter may be shown further by reference to one work—namely that of Iaroslavskii—that it criticized. Stalin did not clearly specify the nature of the errors to which he was alluding, and Iaroslavskii himself seems to have been somewhat baffled. He wrote Stalin several letters requesting clarification but received no answer.¹⁷ In various party discussions prior to the appearance of Stalin's letter, Iaroslavskii had defended every Leninist's right to voice his view on "any controversial question" without fear of being branded a "revisionist."¹⁸ From Stalin's standpoint, such a position was certainly "rotten liberalism" and, hence, an error in principle. As for historical errors, a quick glance through volume four of the party history, covering the period 1917 to 1921 and published under Iaroslavskii's editorship, could have indicated to Iaroslavskii at least one area of difficulty: while poisonously anti-Trotsky in its account, for instance, of Trotsky's position in the Soviet trade-union controversy of 1920, the book treated Trotskyism as the (wrongheaded) faction of Communism that Stalin now said it had "long since" ceased to be; the book did not show Trotskyism to be, even incipiently, the forward detachment of the counterrevolutionary bourgeoisie that Stalin declared it had become. Even the reprinted photographs seemed ill chosen in some cases. Here, for example, was Lenin's original fifteen-man Council of People's Commissars; Trotsky appeared to the left of Lenin (and Alexei Rykov, appropriately, flanked Lenin on the right), while Stalin appeared in the bottom row, next to the Kremlin wall. And here, too, on another page, was an old photograph of the Soviet delegation to the Brest talks, with Trotsky, its leader, looking handsome and impressive in the top row.¹⁹ What Iaroslavskii may have been a little slow in grasping was that affirmation of Stalin necessitated the retrospective denigration of many others who had played more prominent roles in the Revolution than had Stalin.

Further, this volume of the party history made brief reference to the well-known fact, acknowledged by Stalin himself in a speech in 1924, that in March 1917, prior to Lenin's return to Russia and the issuance of his "April Theses," Stalin had shared with Kamenev and M. K. Muranov "an erroneous position" on policy toward the Provisional Government (they had advocated that the party merely put pressure on the government to leave the war). This easily documentable truth of party history as written before 1929 was one of the Iaroslavskii "mistakes" to which Stalin's letter alluded. It became an "un-

¹⁷ *Vsesoiuznoe soveshchaniie o merakh uluchsheniia podgotovki nauchno-pedagogicheskikh kadrov po istoricheskim naukam. 18–21 d-kabria 1962 g.* (Moscow, 1964), 363.

¹⁸ Paul H. Aron, "M. N. Pokrovskii and the Impact of the First Five-Year Plan," in John Shelton Curtiss, ed., *Essays in Russian and Soviet History in Honor of Geroid Tanquary Robinson* (New York, 1962), 301.

¹⁹ E. M. Iaroslavskii, gen. ed., *Istoriia VKP(b)*, 4 (Moscow-Leningrad, 1929): pt. 1, 230, pt. 2, 291. Iaroslavskii explained in his editorial foreword that the volume had been in preparation for the tenth anniversary of the Revolution (1927) "but for a whole series of reasons was delayed for a year." He did not explain what those reasons were.

fact" in party history as rewritten in the 1930s by Iaroslavskii and others. The system of falsification extended to retrospective censorship by or for Stalin of his own earlier writings—the deletion, for example, from later printings of *Problems of Leninism* of Stalin's reference in 1924 to the position he took in March 1917. Subservient writers falsified actual party history in conformity with an idealized image of the "real Bolshevik" for whom straying from the path of revolutionary rectitude was clearly impossible—an image representing Stalin's self-concept. The logical groundwork of this system of falsification was laid in Stalin's letter to *Proletarian Revolution*.²⁰

HELL BROKE LOOSE on the party history and theory fronts as soon as Stalin's letter appeared. The Communist Academy's institutes hastily called meetings to discuss the document's implications for their work. Many editors and scholars were dismissed from their jobs and expelled from the party. *Proletarian Revolution*, after putting out the issue containing the letter, suspended publication in 1932. On reappearing in early 1933, it had a wholly new editorial board, one of whose members was Ivan Tovstukha, Stalin's one-time personal secretary.

Soviet archival sources reveal that all of the Soviet historical journals received instructions to print the text of Stalin's letter and to carry appropriate editorials on its meaning for their respective areas. In a confidential letter of November 26, 1931 to the editorial board of one such journal, *The Class Struggle*, Stalin's erstwhile personal assistant—by then secretary of *Pravda*'s editorial board—L. Z. Mekhlis said that materials in preparation should be written through the prism of Stalin's propositions. The Communist Academy's presidium met on November 31 to review its affiliates' responses to the Stalin letter. K. G. Lur'e, academic secretary of the Society of Marxist Historians, reported that all of the society's sections had been instructed to review the whole literature on the party's history critically in the light of Stalin's "article."²¹ Trotskyist contraband had already been brought to light in numerous works. Many writers, for example, had failed to show the earlier leading role of the Russian Bolsheviks on the international Marxist arena. And Lur'e combined the unmasking of contrabandists with criticism of three well-known party figures—Iaroslavskii, Karl Radek, and I. I. Mints.

Proceedings and reports from other academic groups show that not only historians and their histories but all members and sectors of the theoretical front were being brought into line with higher-level, authoritative interpretation of Stalin's letter. A representative of literary criticism denounced the

²⁰ For a different interpretation of the key purpose of Stalin's letter, see John Barber, "Stalin's Letter to the Editors of *Proletarskaya Revolyutsiya*," *Soviet Studies*, 28 (1976): 21–41. Ignoring the cult question, Barber has suggested that the letter was chiefly occasioned by the "falling quality of party recruits" and an insecure regime's "concern over the tendency of its Marxist intellectuals to engage in too much controversy and speculation," and he has questioned whether the letter was intended to have the effect it did or was conceived as the vital turning point it proved to be. To me Barber's position is unpersuasive.

²¹ *Vsesoiuznoe soveshchanie*, 19, 362, 457, 75. Also see Dunaevskii, "Bol'sheviki i germanskii levye na mezhdunarodnoi arene," 508–09.

"Menshevik-Trotskyist view" of Maxim Gorky's writings, without indicating what that view was, and said that Stalin's letter necessitated criticism of the literary policy—also not identified—of the Second International. A writer named Butaev reported that the Institute of Economics had set up a special brigade to re-examine economic theory in light of Stalin's letter and to "bring to light Trotskyist contraband in the literature on economics." Examples of such contraband were the still-prevalent petty-bourgeois and Trotskyist ideas that equated socialism with equal remuneration and the view, voiced in a book published in 1931, that Henry Ford's factories and assembly lines were a model for Soviet rationalization of labor processes. The legal theorist E. B. Pashukanis, speaking for the Institute of Soviet Construction and Law, criticized a textbook by two authors (one of them Butaev) that contained no account of what Stalin had said in 1927 about the proletarian state. K. V. Ostrovitianov, an economist, objected to the hitherto-accepted notion that the writings of Lenin and Stalin belonged to "politics" as distinct from "economics," whereas in fact they presented the basic laws of socialism's construction and Soviet economic life. Not surprisingly, Ostrovitianov in later years became the Stalin-surrogate for economics.²²

A speaker from the Institute of Technology assailed the "narrow technicism" that he said was characteristic of Trotskyism, condemned the "technological policy of social-fascism," and asserted that a review of "literally the entire technological literature" was now needed. A representative of the Institute of Philosophy, in addition to discussing its new tasks, remarked that the Institute of Technology should produce in short order "a work systematizing all of the basic theses of Marx, Engels, Lenin, and Stalin on technology." The representative of the Association of Natural Science wondered why the basic methodological postulates about physics provided by Lenin in *Materialism and Empirio-Criticism* were not being taken as a guide in an attempt "to create a conception of physics, to produce our Marxist-Leninist conception of the structure of matter."²³ Nadezhda Mandelstam, then working in the editorial offices of the journal *For a Communist Education*, recalled later how "all of the manuscripts were rechecked in great panic and we went through huge piles of them, cutting mercilessly. This was called 'reorganization in the light of Comrade Stalin's remarks.'"²⁴

The pell-mell rush to ferret out "Trotskyist contraband" and "rotten liberalism" was deeply troubling to many in responsible posts, in part, no doubt—but only in part—because of the pressure and embarrassment they themselves were in some cases experiencing. Stalin was not yet an absolute dictator; some in high places failed to realize that he was on the way to

²² According to Katsenelinboigen, "In the forties, K. V. Ostrovitianov was appointed as the curator of economics. All he did was provide commentaries for Stalin's work; he had no opinions of his own, and made no practical recommendations." "Conflicting Trends in Soviet Economics in the Post-Stalin Era," 375.

²³ *Vestnik kommunisticheskoi akademii*, nos. 1–2 (1932): 40–66.

²⁴ Nadezhda Mandelstam, *Hope against Hope: A Memoir*, trans. Max Hayward (New York, 1970), 259. Although she spoke of it as a letter of 1930 in *Bolshevik*, it is clear from the context that Mandelstam was referring to the 1931 letter to *Proletarian Revolution*, also printed in *Bolshevik*.

becoming one or to understand what was driving him to it. Several prominent Old Bolsheviks—including Ol'minskii, Iaroslavskii, V. Knorin, and N. Lur'kin—sought to restrain those “glorifiers” (as Iaroslavskii called them in a handwritten note found decades later in the party archives) who were taking Stalin's letter as a new gospel. Knorin suggested to a meeting of the party group of the Society of Marxist Historians on November 11, 1931 that the letter should simply be seen as a restatement of some basic Leninist tenets. Lur'e, on the other hand, said that party history had lacked all methodology before Stalin's letter appeared and that historians did not grasp the relation between theory and practice. I. I. Mints, who was present at the meeting, wrote a letter to Iaroslavskii, who was out of town, saying that Lur'e, in her “nasty and unsound” speech, had put things less charitably: “Before Stalin's letter there was nothing, and only now does she understand the relation between theory and practice.” Yet three weeks later Lur'e reported to the Communist Academy's presidium on the situation in the Society of Marxist Historians. At about the same time, Iaroslavskii warned against certain unprincipled people who wanted “to make capital on this question” of the Stalin letter. But this statement, along with his handwritten note recalling “how the glorifiers ‘worked me over’ in 1931,” did not see publication until 1966.²⁵

One month after Stalin's letter appeared, his headquarters began to take action against those who pleaded for restraint. Lazar Kaganovich gave a long speech at the Institute of Red Professors on December 1, 1931—the occasion of its tenth anniversary. When the text appeared in *Pravda* some days later, it became clear that the address was meant to reach the whole Soviet intelligentsia. But “address” is a misnomer. The document is best described as a several-thousand-word, peremptory command by drill sergeant Kaganovich ordering the army of the intelligentsia to snap to attention in the light of General Stalin's letter.

Kaganovich introduced his discussion of the letter by stressing the great importance of Marxist-Leninist indoctrination at a time when individuals who had only been members of the party for three to five years comprised one and a half to two million out of a total of two and a half million party members and when the Komsomol numbered five and a half million Young Communists. No one in the party would have disputed the statistics and their general implications, but Kaganovich quickly made it clear that what was at issue was the specific content of party indoctrination. The millions of new members must learn that, if the country once thought the most backward in the world was now the land of socialism, “We owe this to the selfless struggle waged for decades by the best people, headed by Lenin, against the *narodniki*, legal Marxists, economists, Mensheviks, Trotskyists, rightists, and conciliatory elements in the party.” Clearly, Stalin was the best of “the best people.” Kaganovich then spoke of the “criminality” of slanderer-falsifiers like Slutskii.

²⁵ Dunaevskii, “Bol'sheviki i germanskii levye na mezhdunarodnoi arene,” 509–12. The Russian word here translated as “glorifiers” is *alliluishchiki*.

Radek, Kaganovich continued, had acknowledged his own errors to the party group of the Society of Marxist Historians: he had recognized, furthermore, that Rosa Luxemburg did not always take "a correct Bolshevik position" but had argued that Rosa was a "bridge" to Bolshevism for the best Social Democratic workers. In fact, Kaganovich charged, Radek himself had been a bridge between Rosa Luxemburg and Trotsky.

The importance of Stalin's letter, Kaganovich said, did not lie in its attack on the insignificant ex-Menshevik Slutskii, whom Stalin had pulverized in passing, but in exposing the rotten liberalism shown by the editors of *Proletarian Revolution* toward deviations from Bolshevism and distortions of party history. And this journal was not the only weak spot. A still weaker one was Comrade Iaroslavskii's four-volume history, criticism of the errors of which would "undoubtedly develop further." Among his illustrations of the history's grave errors, Kaganovich mentioned its "erroneous and harmful assessment of the role of the Bolsheviks in the first period of 1917, [its] foul slander of the Bolsheviks." Kaganovich delivered this veiled rebuke to Iaroslavskii for his reference to Stalin's "erroneous position" in March 1917. Then came a methodological pointer: the key to a comprehensive party history was the "flexibility of Lenin's tactics," not passages in which Lenin said, in so many words, "Kautsky is a bastard." What, in short, a "real Bolshevik" said or failed to say at a particular time was not the touchstone of party-historical truth; the documents must be interpreted according to the canons of the real-Bolshevik-revolutionary-can-do-no-wrong school.

Kaganovich ended with an implicit call for an intensification of the ongoing hunt for heresy. Difficulties were rife, the fight was not over, the class struggle was continuing. "Opportunism is now trying to creep into our ranks, covering itself up, embellishing itself, crawling on its belly, trying to penetrate into crannies, and trying, in particular, to crawl through the gates of the history of our party." In his recent speech Radek was wrong to describe the Comintern as a channel through which many different currents and brooklets flowed into the Bolshevik party. The party was no meeting place of turbid brooklets but a "monolithic stream" capable of smashing all obstacles in its path. The meaning was as clear as the metaphor was mixed: fall in line or be destroyed.²⁶

The pleaders for restraint—and others—fell into line. Within the twelve days following Kaganovich's speech of December 1, *Pravda* carried letters of recantation from Radek, Iaroslavskii, and the party historian Konstantin Popov. Radek pleaded guilty to all of Kaganovich's charges and joined the attack on "Luxemburgianism." Iaroslavskii acknowledged a whole series of "the grossest mistakes" in the four-volume history, including "an objective, essentially Trotskyist treatment of the Bolsheviks' position in the February-March period of the Revolution of 1917" (Trotskyist, presumably, because

²⁶ *Pravda*, December 12, 1931. Dunaevskii has observed that "Kaganovich's speech, filled with shouted threats, was designed to pin the label of Trotskyist on all from now on who would dare to deviate from Stalin's propositions"; "Bol'sheviki i germanskii levye na mezhdunarodnoi arene," 511.

Trotsky was one of those who had called attention to the generally known facts about Stalin's position at that time). He also disavowed the view, reportedly expressed by Mints in a recent speech, that the authors of the four-volume history had erred in their objectivity and that what was now being asked of party historians was "not so much objectivity as political expediency." No, lied Iaroslavskii, the party had not and could not demand that historians surrender their objectivity; the problem was that the authors of the four-volume work had sinned against objectivity.²⁷ Resigning himself to the situation, Iaroslavskii started work on the glorifying biography of Stalin that was published in 1939.

Plainly, to confess to heresy was not enough; the heretic had to join the inquisition. Only by entering the ranks of the accusers could he expect to have his recantation taken seriously. To denounce Trotskyist contraband on the part of others demonstrated the genuineness of one's own "real" Bolshevism—that is, Stalinism. Recantation followed by denunciation was becoming a ritual of Soviet political culture. Iaroslavskii's public disavowal of his friend Mints was but one of many examples.

Still, Stalin did not yet wield absolute power. Those higher in the hierarchy of power than Iaroslavskii could suggest the need for restraint. Among them was P. P. Postyshev, then a full member of the party Central Committee, a member of its Orgburo, and one of four Central Committee secretaries serving under General Secretary Stalin. As a secretary, Postyshev was in charge of the Central Committee's Organizational Department and its Department of Agitation and Propaganda, whose functions included oversight of the press. In a speech at a district party conference in Moscow, he stressed the great significance of Stalin's letter and then took various party cells to task for their failure to distinguish between an individual's particular mistakes and a "system of views." Of course, there were concealed Trotskyists in the party's ranks, who must be exposed and expelled. But there were also comrades who had simply erred. Instead of denouncing them as deviationists and kicking them out of the party—as did some who had been asleep but now wanted to "show themselves" (and then go back to sleep)—errant comrades should be criticized in a comradely way. Postyshev's fate after trying to curb the excesses of the heresy hunt was instructive: arrested in 1938, he was killed in 1940 in one of Stalin's concentration camps.²⁸

The master-builder of the Stalin cult was the cult-object himself. But many others, ranging from men in Stalin's entourage like Kaganovich and Mekhlis to obscure ideological workers like Lur'e, assisted. Who, we may now ask, were the glorifiers? Some, without doubt, were persons devoted to Stalin or to the man they idealistically perceived him to be; others were simply careerists who may have lacked strong qualification in intellectual work but who were shrewd or, perhaps, cynical enough to grasp the opportunities for self-ad-

²⁷ Iaroslavskii's letter appeared in *Pravda* on December 10, 1932; Radek's on December 12; Popov's on December 8.

²⁸ T. Mariagin, *Postyshev* (Moscow, 1965), 299–300. The speech in question was reported in *Pravda* on January 11, 1932.

vancement inherent in the Stalin-glorifying enterprise. One climber who made his way to the top by this route was the head of the Georgian secret police, Lavrentii Beria, who with Stalin's backing became party chief of the Transcaucasus in 1932. The one indispensable quality shared by all of the glorifiers, high and low, was pliability. In very many ways the aggrandizement of Stalin required the twisting of truth and the falsification of historical fact. As Iaroslavskii himself expressed it, the glorifiers had to be "unprincipled," pliable enough to ignore their scruples and still their consciences insofar as the cult-building enterprise required.

THE LETTER TO *Proletarian Revolution* was a turning point in the cult's evolution. From the time of its appearance forward, idolatry of Stalin became one of Russia's major growth industries. No field of Soviet culture was exempted from finding inspiration for its activities in Stalin's letter. The journal *For Proletarian Music*, for example, devoted its editorial in January 1932 to "Our Tasks on the Musical Front" in light of the letter, and the corresponding editorial in the February 1932 issue of *For a Socialist Accounting* bore the title, "For Bolshevik Vigilance on the Book-Keeping Theory Front." But revolutionary history and Stalin's place in it remained the central concern. A small example, typical of many, was an article published in *Pravda* shortly after Stalin's letter appeared. It denounced a book on Comintern history on the grounds that Stalin's name was only mentioned twice and said, "Without showing Comrade Stalin's leading role in the history of the Comintern, there can be no Bolshevik textbook on the history of the Comintern."²⁹

Having asserted himself as premier party historian, Stalin delivered another lecture in reply to two party members, Olekhovich and Aristov, who had written separately to him in response to the letter; and his answers, dated January 15 and 25, 1932, were published in *Bolshevik* (and then in other publications) the following August. Olekhovich, apparently, had tried to show himself more Stalinist than Stalin and suggested that "Trotskyism *never* was a faction of Communism" but "was *all the time* a faction of Menshevism," although for a certain period of time the Communist Party had wrongly regarded Trotsky and the Trotskyists as real Bolsheviks. In knocking this construction down, Stalin showed the hair-splitting quality of his mind. Undeniably, he said, Trotskyism was once a faction of Communism but oscillated continually between Bolshevism and Menshevism; even when the Trotskyists did belong to the Bolshevik party, they "were not *real* Bolsheviks." Thus, "in actual fact, Trotskyism was a faction of Menshevism before the Trotskyists joined our party, temporarily became a faction of Communism after the Trotskyists entered our party, and again became a faction of Menshevism after the Trotskyists were banished from our party. 'The dog went back to its puke.'"³⁰

²⁹ *Pravda*, December 29, 1931.

³⁰ Stalin, *Sochineniia*, 13: 126-30.

These further pronouncements only confirmed to professionals that they should look to Stalin's writings and sayings as scripture. As if to meet their need, party publications in 1932 started printing early Staliniana, such as Stalin's virtually unknown letter of 1910 to Lenin from Sol'vechegodsk exile and his little-known "Letters from the Caucasus" of that same year. Meanwhile, the glorifiers set about rewriting history in accordance with Stalin's canons and in a manner calculated to accentuate his role and merits in the party's revolutionary past, while discrediting those of his enemies. The skewed Stalinist version of Bolshevism's biography began to emerge. Grosser falsification still lay ahead.

The rise of the Stalin cult did not bring the eclipse of the Lenin cult, only its far-reaching modification. Instead of two cults in juxtaposition, there emerged a hyphenate cult of an infallible Lenin-Stalin. In some respects, Lenin now "grew" in stature: he became the original "real Bolshevik" who could not have erred. But by being tied like a Siamese twin to his successor, he was inescapably diminished in certain ways. Only those facets of his life and work that could be connected with Stalin's were available for full-scale idealization, and whatever did not in some way include Stalin had to be kept in the background. In effect, some parts of Lenin's life had to be de-emphasized and others rearranged, modified, or touched up to put Stalin in the idealized picture.

Thus, Stalin was now portrayed as sharing in Lenin's exploits, was declared to be from an early time Lenin's right-hand man, on whom the leader leaned for counsel and support at key points in the development of the Revolution and after. The marking on May 5, 1932 of the twentieth anniversary of *Pravda's* founding may be taken as an illustration. At the beginning, said *Pravda's* anniversary editorial, Lenin "wrote articles for the paper nearly every day—with the closest participation and guidance of Comrade *Stalin*, particularly when Lenin was hiding underground." So in the dual cult the younger figure emerged as Lenin's alter ego, who naturally took over when Lenin himself was away from the immediate scene of action. Symptomatically, the article was accompanied by a large portrait not of Lenin but of Stalin and contained a lengthy quotation from Stalin's recollection of 1922 on the paper's early days.

By now Iaroslavskii had not simply fallen in line but had joined the vanguard of the glorifiers. Invited to contribute an article in commemoration of the twentieth anniversary of the Prague Conference of January 1912, he found a shrewd way of enthroning Stalin in retrospect practically as a founder of the Bolshevik party. As Lenin had testified, Bolshevism had existed as a political current from 1903, when the Bolshevik-Menshevik schism occurred at the Russian Marxist party's Second Congress. But the Bolshevik Party's formal existence dated only from the all-Bolshevik Prague Conference of 1912, at which Lenin converted what had been a faction into a separate party no longer organizationally tied to the Mensheviks. In the aftermath of the Prague Conference Stalin was elevated (by cooptation, not election) for the first time to membership in the party's Central Committee. Iaroslavskii obscured the

embarrassing fact of Stalin's co-optation by saying, "At the conference a Bolshevik Central Committee was elected in the persons of Lenin, Stalin, Zinoviev, Ordzhonikidze, Belostotskii, Shvartsman, Goloshchekin, Spandarian, and Ia. M. Sverdlov (some of these comrades were co-opted into the Central Committee subsequently)." And by writing with heavy emphasis—"The Prague Conference was a *turning point in the history of the Bolshevik Party*"—he contrived to portray Stalin by indirection as having been present at the party's creation.³¹

Even clever party theorists were in some cases slow in comprehending the transformed personality cult and in applying its special canons. One person who illustrates the early confusion was S. E. Sef, a zealous glorifier, who was managing secretary of the journal *Marxist Historian*. He gave the provisional title "Marx, Engels, Stalin" to the lead article of a planned special issue commemorating the upcoming fiftieth anniversary, in March 1933, of the death of Marx. His omission of Lenin was corrected before the issue appeared.³² Sef had failed to grasp that Lenin *qua* co-leader remained a cult-object. In the dual cult, however, the figure of the successor in some ways now began to tower over that of the predecessor. For example, a foreign correspondent's count of "political icons" (portraits and busts of leaders) in display windows along several blocks of Moscow's Gorky Street on November 7, 1933 showed Stalin leading Lenin by 103 to 58.³³

Stalin was now being sung, especially by poets from the Orient, where versified flattery of rulers is a centuries-old art. "To the *Vozhd'*, to Comrade Stalin" was the title of a long poem by A. A. Lakhuti, translated from Persian into Russian. A typical stanza reads,

Wise master, Marxist gardener!
Thou art tending the vine of communism.
Thou art cultivating it to perfection.
After Lenin, *vozhd'* of Leninists.³⁴

Meanwhile, scholars in Oriental studies were enjoined to apply the works of Stalin as well as those of Lenin to problems of the national-colonial revolution in the East. A pamphlet on the history of the Georgian Communist Party was attacked for treating the period from 1917 to 1927 in a spirit of "national deviationism" (that is, Georgian nationalism) contrary to Stalin's orientation; and among those who were later reported from Tbilisi to have condemned the offensive pamphlet was Lavrentii Beria.³⁵ Stalin's early revolutionary years in Transcaucasia now began to attract reverent attention. A pamphlet published in Georgia portrayed the young Stalin as a heroic leader directing underground revolutionary activities in Batumi in 1901–02.³⁶

³¹ *Pravda*, January 22, 1932.

³² Dunaevskii, "Bol'sheviki i germanskii levye na mezhdunarodnoi arene," 511–12.

³³ Eugene Lyons, *Moscow Carrousel* (New York, 1935), 140–41.

³⁴ *Pravda*, November 29, 1932. Iranian by origin, Lakhuti had emigrated to the USSR and become a Soviet citizen.

³⁵ *Pravda*, March 21 and 25, 1932.

³⁶ *Stalin i Khashim (1901–1902 gody): Nekotorye epizody iz batumskogo podpol'ia* (Sukhum, 1934).

The cult kept growing in official publicity during 1933. *Pravda* marked the fiftieth anniversary of Marx's death on March 14 by lauding Stalin's theoretical contributions to materialist dialectics and concluded, "Stalin's name ranks with the great names of the theoreticians and leaders of the world proletariat—Marx, Engels, and Lenin." The phrase "classical works of Marx, Engels, Lenin, and Stalin" was now commonplace. Partizdat, the party publishing house, was savagely criticized for its failure to eliminate a series of minor misprints in the latest printing of the fastest selling of the classics, Stalin's *Problems of Leninism*. "As if 'minor' misprints are allowable in a book by Comrade Stalin!" the critic parenthetically exclaimed.³⁷ Overall figures released in early 1934 show that the classics had been published in 1932–33 in the following numbers: seven million copies of the works of Marx and Engels, fourteen million of those of Lenin, and sixteen and a half million of those of Stalin, including two million copies of *Problems of Leninism*.³⁸ That collection of Stalin's articles and speeches was by then well on the way to becoming probably the world's best seller of the second quarter of the twentieth century.³⁹

From that time forward, to the end of Stalin's life, his aggrandizement through the personality cult continued incessantly.

³⁷ *Pravda*, February 22, 1933.

³⁸ XVII s'ezd vsesoiuznoi kommunisticheskoi partii (b) 26 ianvaria–10 fevralia 1934 g. *Stenograficheskii otchet* (Moscow, 1934), 620.

³⁹ By 1949 almost seventeen million copies in fifty-two languages were in print. See *Bol'shevik*, no. 23, December 1949, p. 48.

What Fascism Is Not:
Thoughts on the Deflation of a Concept

GILBERT ALLARDYCE

"PERHAPS THE WORD FASCISM SHOULD BE BANNED, at least temporarily, from our political vocabulary," S. J. Woolf wrote in 1968.¹ Historians who have confronted the problem of defining this mulish concept may sympathize with this modest proposal. Unfortunately, the word "fascism" is here to stay; only its meaning seems to have been banned. Nevertheless, the German philosopher-historian Ernst Nolte is probably correct in stressing that historians do not have the responsibility to invent new terms simply because the existing ones seem inadequate. But they do have the responsibility to confess how truly inadequate the term fascism has become: put simply, we have agreed to use the word without agreeing on how to define it. This article is concerned with the reasons for this unfortunate state of affairs.

Although some scholars attempted from the start to restrict the use of the term fascism to Mussolini's movement in Italy, most have joined in a process of proliferation that began as early as the 1920s. After Mussolini's success, observers thought they recognized men and organizations of the same type arising in other nations. From this beginning emerged a popular image of fascism as an international movement, a phenomenon that found purest expression in Italy and Germany, but also appeared in a wide number of other countries. When stripped of national trappings, it is commonly believed, all of these movements had a common characteristic that was the essence of fascism itself. Although that essence is difficult to define, the prevailing hope is that continuing research will eventually reveal the nature of facism more clearly. Thus, while the thing itself continues to elude us, the name goes on as before.

Edward R. Tannenbaum has observed that the study of fascism appears to instill in scholars a particular compulsion for reductive logic, a tendency to

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¹ Woolf, Introduction to S. J. Woolf, ed., *European Fascism* (New York, 1969), 1.

relate the phenomenon to a single and central significance.² They usually “reduce” it, however, to a significance that is very large indeed. From the beginning writers seem to have felt compelled to deepen fascism’s importance, to “see through” its ideas and rationalizations, and to explain it in terms beyond the fascists themselves. The movement appeared simply too aberrant, too demagogic, too empty of ideas and honest motives to be taken at face value: fascism had to be “deciphered.” Ernst Nolte has noted that, when Mussolini deserted socialism to support Italian intervention in the First World War, his old Marxist comrades immediately asked a question that was elaborated thereafter into the socialist conception of fascism: “Who is paying?”³ Thus was created, virtually by reflex, the first interpretation of fascism. Others followed, but most continued in the same way to conceive of fascism as an agent of someone or something and invariably portrayed it in literature written between the world wars as a force in itself brutal, opportunistic, and unintelligent, but momentous in what it revealed of the society that gave it birth. In large part, contemporary historians have continued this process of intellectual inflation. Whether they envision fascism as the tool of class interests or the expression of more impersonal forces—the revolt of the masses, the moral crisis of civilization, totalitarianism, or the modernization process—they generally understand it in terms of something more fundamental and important to history. Rarely, on the other hand, is it understood in terms of what the “fascists” themselves declared it to be. Communist ideas are interpreted seriously, complains one political scientist; fascist ideas are merely “interpreted.”⁴

This elevation of fascism as a historical concept places considerable strain on the rules of evidence. The burdens of research necessarily restrict the number of personalities and organizations that historians of fascism can investigate with sufficient competence and depth, and thus the effort to develop general theories inevitably carries them beyond the limits of specific knowledge. Unfortunately, the diversity of these personalities and organizations is such that general theories formed from the study of certain samples are often contradicted by the study of others. The more we know in detail, the less we know in general. Few historians, however, have lost confidence that further research will unearth the “missing link” that unites the different individuals and parties in a generic fascism. Somewhat like the search for the black cat in a dark room, this search presumes that there is something to be found in the dark void. In this sense the notion of generic fascism exists in faith and is pursued by reason. In a way, the problem is reminiscent of the philosopher’s world of universal forms and real objects. If so, few concepts are more in need of Ockham’s razor than fascism. Only individual things are real; everything abstracted from them, whether concepts or universals, exists solely in the mind. There is no such *thing* as fascism. There are only the men and movements that we call by that name.

² Tannenbaum, *The Fascist Experience: Italian Society and Culture, 1922–1945* (New York, 1972), 3.

³ Nolte, *Three Faces of Fascism*, trans. Leila Vennewitz (New York, 1966), 16.

⁴ A. James Gregor, *The Ideology of Fascism* (New York, 1969), 14.

In these pages I have used—some will say abused—the term fascism in this way. Critics may judge that this is too facile and that it trivializes the problem of fascism into a kind of word game, contributing nothing toward a final solution, neither defining the concept nor doing away with it. I have explained that historians cannot do either. Fascism at present is too hot to handle. I hope to cool it down. The premise of this article is that our understanding of the real men and movements that we call fascist has not been increased by generic concepts. Instead, general definitions have probably obscured their individual identities. To recognize the variety of fascism is to recognize the need to free it from the tyranny of concepts. What follows is a move in that direction, a criticism of three conceptions of fascism that have been developed most completely in English-language works and that, therefore, dominate our thought on the subject: (1) fascism as a generic concept, (2) fascism as a political ideology, and (3) fascism as a personality type.

Admittedly, the three popular conceptions described above hardly exhaust the range of historical reflections on fascism. Other interpretations exist in considerable number, and therefore the results of this investigation require qualification. The premises inherent in these three conceptions, however, often underlie or otherwise give substance to most other explanations. Certainly independent versions exist, and more keep coming. Indeed, books and articles that describe the competing interpretations of fascism have become a burgeoning industry, and new conceptions continue to advance from all sides. By contrast, my purpose is to “deconceptualize” fascism, to ask not what it is as a collective entity, but what it is not.

I should mention at this point, by way of an aside, that Marxist interpretations of fascism (there are a variety) will not be discussed here. Most of the older writings in this tradition reveal the impoverishment of Marxist historiography rather than the character of “fascist” movements. Meanwhile, new concepts, still largely in the process of formulation, remain too adventurous and immature. A new generation of scholars on the Left, straining for light and air, have raised the level of investigation from the economic base of capitalism to its ideological superstructure, renouncing vulgar Marxist economic determinism in favor of a more sophisticated cultural analysis of bourgeois society. But the old premise remains: the study of fascism begins with the study of capitalism. Perhaps some of these young researchers would be better advised to begin with the study of communism instead. Their project to revise the interpretation of fascism, which they inherited from the Third International, would, no doubt, benefit from a thorough review of just how Marxists derived such a rigid and fatalistic conception in the first place.⁵

⁵ A recent book by Nicos Poulantzas represents a decidedly left-footed step in this direction. Published originally in French in 1970, and still crackling with the barricades spirit of 1968, the work attempted to rectify the “errors” of the Comintern line on fascism. One may hope that this de-Stalinization of the Communist view will encourage other dissidents to go beyond Poulantzas and to de-Leninize it as well. Nonetheless, the book is informative in revealing the trend of new European Marxist literature on fascism toward a conception less universal, less “predestined,” more historical—and just as didactic as ever; Poulantzas, *Fascism and Dictatorship: The Third International and the Problem of Fascism*, trans. Judith White (London, 1974).

FIRST OF ALL, FASCISM IS NOT A GENERIC CONCEPT. The word *fascismo* has no meaning beyond Italy. Yet it was applied from the beginning to movements that arose in other nations, movements whose fate it was to be interpreted in terms of Mussolini's organization. Such parties presumably corresponded to foreign "models," first the Blackshirts, and later the Nazis. "They claim that we are fascists, but they know that this is a lie," protested Jacques Doriot, the leader of the *Parti populaire français*, in 1937. "We do not think that the regime of Hitler or Mussolini can be fitted to our country."⁶ Such men, however, could no more get rid of the word in their time than historians can be rid of it today. Even those outside Germany and Italy who adopted the term for their own political purposes came to recognize the curse of its association with things foreign. Oswald Mosley, leader of the British Union of Fascists, contended that his own movement was a form of English patriotism that became so confused in the public mind with alien powers that it was denounced as unpatriotic. Students of politics, of course, must always distinguish between movements already in control of the state and those still competing for power. Few were more aware of the difference than the so-called fascists themselves. Mosley complained in later life that, whenever some success appeared possible in the 1930s, his organization received a "knock-down blow" from the actions of Hitler and Mussolini themselves, blustering into some new European crisis for purely national interests and spreading alarm and opposition in England.⁷ "Patriotic traitors" like Mosley and Doriot were alternately inspired and anguished by the two dictators, imitating them at one minute and denying their influence at the next. On the one hand, they drew support from the secret subsidies of Rome and Berlin; on the other, they suffered the hatred of a public enraged by Italian and German aggression. In this sense, international relations between such leaders were always "brutal friendships."

That the slogan, "nationalists of the world unite," involves a logical impossibility is a common observation. In the 1930s, however, ideological battles had a logic of their own. Between 1933 and 1936, when Rome and Berlin competed for influence over organizations in other countries and when Mussolini stood guard at the Brenner Pass to prevent Nazi expansion into Austria, the popular image of fascism remained largely bipolar. Fascists were either Blackshirts or Nazis: they looked for inspiration either to Rome or to Berlin. With the Axis alliance, however, rivalries became blurred; and, during the ideological crusade that began with the Spanish Civil War, antifascists everywhere created a fascist model of their own—an international fascism of jackboots, barbed wire, and corpses: irrational, anti-Semitic, totalitarian, and genocidal. Fascists were fascists, and Mussolini and Hitler were their prophets. Elsewhere I have observed how the changing interpretations of fascism reflect the intellectual illusions of the periods that produce them.⁸ During the

⁶ *La Liberté* (PPF newspaper), June 25, 1937.

⁷ Mosley, *My Life* (London, 1968), 292.

⁸ See my introduction to Gilbert Allardyce, ed., *The Place of Fascism in European History* (Englewood Cliffs, N.J., 1971), 1–27.

Cold War, scholars transformed this propaganda image of a monolithic and murderous fascism into the concept of "totalitarianism," an ideological construct developed from the comparative study of the Stalinist and Nazi regimes and extended on the barest evidence to Communists and "fascists" across the globe. Recently, further investigation has undermined not only this conceptual unity of Nazism and Stalinism as totalitarian systems, but the conceptual unity of Nazism and Italian fascism as well. In a seminal work published in 1964, Eugen Weber identified Italian fascism and National Socialism as distinctive expressions and proceeded to classify movements elsewhere in relation to these two "prototypes."⁹ The study of fascism thus became again the study of fascisms, and scholars thought once more in terms of two models rather than one.

The result, however, was merely to re-create in more precise form the bipolar vision of the past. Although earlier political stereotypes now wore the guise of elaborate social science "models" and "paradigms," the premise remained the same: the definition of fascism was established from Italian and German experiences and transferred wholesale to movements in other locations. Many who have studied these other movements, however, sense that something is not quite right; the models do not fit precisely. Indeed, the unique and "native" features of such movements are easy to discover; the difficulty lies in finding the common "fascist" substance that connects them with the Italian or German versions. Weber himself, for instance, has brilliantly illustrated how what he has identified as Rumanian fascism, arising in less complex social circumstances, assumed a form different from the movements in Western Europe.¹⁰ After examining the "native" features of other East European organizations, Peter Sugar concluded that such groups would probably have emerged in that area even if the Blackshirts and Nazis had never existed.¹¹ Those we call fascists identified themselves everywhere with the history, traditions, and symbols of their nations. To impose external models upon them is to repress what they themselves attempted to emphasize. And what is gained? It must be admitted that this method has not convincingly revealed the universal fascist fundamentals that supposedly underlay the nationalist superstructures of the various movements. Nor has it effectively limited the proliferation of fascisms called by other names: clerico-fascism, agrarian fascism, anarchofascism, or the numerous other hybrid forms. Such qualifying adjectives and prefixes reveal the degree to which the two classic models are inadequate to describe the variety of all the movements involved.

The present, foundering debate on fascism and the modernization process indicates just how inadequate these typologies have become. In 1972 Henry A. Turner proposed a new approach to fascism, based on the social science theory of modernization, as the next task for historians. "According to this

⁹ Weber, *Varieties of Fascism* (Princeton, 1964).

¹⁰ Weber, "The Men of the Archangel," *Journal of Contemporary History*, 1 (1966): 101-26.

¹¹ Sugar, *Native Fascism in the Successor States, 1918-1945* (Santa Barbara, Calif., 1971), 156.

theory," he explained, "the one underlying development of recent history is the displacement of traditional societies by an unprecedentedly thorough and rapid process of change, basically similar everywhere, involving industrialization, urbanization, secularization, and rationalization."¹² Five years earlier, A. F. K. Organski had sounded the same call among social scientists, proclaiming that preliminary results already promised a "much deeper understanding of fascist systems."¹³ The first problem here, however, is to understand modernization theory. As with many attempts to comprehend one concept in terms of another, this one tends toward vagueness on both fronts. In effect, there is no modernization theory. Rather there are modernization *theories*, ranging from a Whig history of material "progress" to more remarkable social science formulations developed to analyze the interrelated, long-term evolution of society, mind, and market. When confined to the cramped time span of the fascist "era," however, even the best evolutionary theorems seem to loosen from the events they explain; the connection between mental and material life becomes uncertain, and the differences between friends and enemies of modernity become confused. Perhaps in consequence, scholars often reduce the modernization process to urban and industrial development and measure it in terms of ingots, gadgets, and GNP. They frequently envision the forces opposed to modernization, on the other hand, in wholly ideological terms, and identify them as a collection of mental irrationalities that war against social change and the economic efficiency necessary to higher stages of advancement. Modernization theorists, however, confront a difficult problem in dealing with Nazism and Italian fascism: one arose in the most advanced industrial nation in Western Europe; the other, in a country still largely underdeveloped. Getting both into any uniform theory is hard enough, but getting both into the same stage of modernization is impossible. Interpretations that make sense in the case of one regime often make no sense in the case of the other. In the face of this prospect, the best advice among modernization theorists seems to be: every man to his own devices.

For his part, Organski resolved the problem with a stroke of the pen. Nazism, he concluded, was not fascism. To his mind, fascism was a developmental stage in the modernization process, an elite dictatorship that advanced and industrialized a nation's economy. His model was the reign of Mussolini; the odd man out was Hitler. "Hitler," he wrote, "was an authoritarian dictator, a nationalist, an aggressor, a represser, and a madman, but he was not a fascist, for Germany was fully industrialized when Hitler came to power."¹⁴ The approach here is familiar in fascism studies. First, one works out a definition of a particular movement, the movement then becomes the model, and the definition is used to exclude from fascism those who do not conform. In most cases, the result is a definition lean and pure—but somehow irrelevant. For Organski, however, the exclusion of Nazism is obviously a

¹² Turner, "Fascism and Modernization," *World Politics*, 24 (1972): 558.

¹³ Organski, "Fascism and Modernization," in S. J. Woolf, ed., *The Nature of Fascism* (London, 1968), 21.

¹⁴ Organski, *The Stages of Political Development* (New York, 1965), 123.

logical necessity. Where fascism is defined as a modernizing dictatorship in underdeveloped nations, Nazism must be disposed of in one way or another.

Not everyone, of course, uses such arbitrary methods. There are those who recognize that, when Nazism cannot be explained in one way in modernization theory, it can be explained in another. Alan Cassels, for instance, made Hitler's regime the model of antimodernist fascism, "a blind, nihilistic fury directed against modernism in nearly all its forms." At its core, he asserted, was a primitive racism that generated a politics of blood, instinct, and reaction. The result was an "aversion to modernity," in contrast to the industrializing impulse of Mussolini's corporate state. What emerged were two models of fascism, modern and antimodern, rational and irrational, corporative and *völkisch*. This established, Cassels then extended the same divide-and-conquer strategy to movements throughout Europe. "On the basis of this Nazi-Fascist contrast," he wrote, "I have postulated a general schema: that fascism where it appeared in less advanced regions tended to look ahead to a stepped-up modernization of the community . . . while in already modernized nations fascism preferred to look back to a legendary past."¹⁵

Would that it were so easy. Cassels himself confessed that the pattern did not hold in Eastern Europe, though he believed he could patch up the difference. Others will find his problems more widespread. It is easy to see exceptions to his models elsewhere in Europe (to dismiss Oswald Mosely as an enemy of modernism, for example, is unreasonable), and it is not difficult to object to the models themselves. After laboring mightily to demonstrate that corporatism was the determining element in the progressive nature of Italian fascism, Cassels conceded on his own that the whole corporatist project amounted in truth to little more than a "propaganda exercise."¹⁶ But Nazism, as always, contained the real knots. The difficulty can be simply stated: although consistent with modernization theory, the interpretation of National Socialism as anti-industrialism is not consistent with real events in Germany. In fact, the Third Reich did little to inhibit industry and much to promote it. Cassels's explanation of this "paradox" will not convince everyone:

The crux of the matter was that Hitler required a highly industrialized and urbanized society to sustain rearmament and an efficient war machine. Beyond Germany's frontiers it might be possible to implement some fantasies of village life, but to conquer territory for such experimentation, it was necessary for Germany to become more than ever a modern, mechanized power. Many high-ranking Nazis . . . evinced a childlike fascination with the products of the assembly line—airplanes, cars, gadgets of all kinds. The possession of mechanical objects confirmed their power but signified no commitments to the spiritual values of technocracy.¹⁷

The conclusion, apparently, is that the Nazis were antimodernist in mind but not in practice. Their goal in the future was to "demodernize" Europe; their

¹⁵ Alan Cassels, *Fascism* (New York, 1973), x. For Cassels's original proposal of this schema, see his "Janus: The Two Faces of Fascism," in Canadian Historical Association, *Historical Papers* (1969), 166–84.

¹⁶ Cassels, *Fascism*, 347, and "Janus," 74.

¹⁷ Cassels, *Fascism*, 150.

means in the present was to industrialize Germany. Here one may ask, if antimodernists modernize, are they antimodernists? Never mind. It is sufficient to recognize that National Socialism is as troublesome to interpret with modernization theory as it is without it.

Cassels's premise was that fascism cannot be incorporated *en bloc* into modernization theory. Instead, the manifest difference between Nazism and Italian fascism indicates the need for both "regressive" and "progressive" prototypes. It is confusing enough when scholars debate whether fascism is modern or antimodern; it is worse when someone claims that it is both. In the space of two paragraphs, for example, Cassels described fascism variously as "an onslaught on the nineteenth century," an effort to restore "the vanished world of 1789," "a general twentieth-century phenomenon of the Left," a movement committed to "sweeping away the debris of all anciens regimes," a theory of "socialism . . . applied by authoritarian means," "leftist," "rightist," and "radical."¹⁸ This is what Turner warned against in his appeal to historians of fascism to take up modernization theory in the first place. If the two primary forms of fascism are fundamentally different in relation to the modernization process, he remarked, the concept of generic fascism itself falls apart.

With this in mind, Turner ventured an interpretation of his own, conceived largely from his personal research in German history but extended on credit to Mussolini's regime as well. Both governments, he proposed, represented "utopian forms of antimodernism," a defense of hearth, heroism, and instinct against the onslaught of industry and urban life. As to the "paradox" of Nazi industrialization, Turner agreed with Cassels; further economic development was necessary to achieve the military victory that would make possible the eventual dismantling of the modern world. "The Nazis," he wrote, "practiced modernization inadvertently in order to pursue their fundamentally antimodernist aims."¹⁹ If the reasoning here seems forced, one point at least is easily understood. The effort to interpret Nazism in relation to the modernization process involves a logical imperative. If Hitler's movement is to have a place in modernization theory, it must be as a form of antimodernism. A modernizing movement, it seems, can have no place in the most advanced industrial nation of Europe. Thus, the view from Berlin is clear; fascism is antimodernism. Regrettably for some, however, the view from Rome looks off in the other direction.

Although making his case with evidence from the German regime, Turner hoped for the best from those studying the Italian experience. In response, A. James Gregor delivered a dissenting opinion: "Italian Fascism, no matter what else it was, seems to have been an industrializing and modernizing movement in both performance and intention."²⁰ According to his evidence, Mussolini really did run the trains on time—and the rest of the economy as

¹⁸ *Ibid.*, 344–45.

¹⁹ Turner, "Fascism and Modernization," 538.

²⁰ Gregor, "Fascism and Modernization: Some Addenda," *World Politics*, 26 (1974): 382.

well. Knowing with whom to agree would help, but the truth in this matter is likely to remain as imprecise as modernization theory and the meaning of "modernity" itself. Where one begins the study of fascism often determines where one arrives at the end. Gregor began at the beginning. The study of fascism, he reasoned, commenced in name and content with the appearance of Mussolini's organization. All definitions, therefore, should be judged at the outset by their ability to explain this "unambiguous instance" of fascism in real life. Gregor's result was not unexpected: for him, the "maximally plausible" explanation was that fascism was an episode in the drive toward modernization.²¹

No one has thought longer and harder than Gregor about the problem of fascism and the modernization process. Therefore, he is aware of his misfortune: Nazism does not fit his theory. Rather than deny the fascist identity of Hitler's movement, however, he deemed it a special case, a fascism "anomalous" and "unique," a system that was born and died with the Führer himself.²² Through a succession of impressive books on fascism as "a developmental dictatorship appropriate to partially developed or underdeveloped . . . national communities," Gregor has admitted frankly that National Socialism "bedevils" his analysis.²³ One can only admire his honesty—and deplore his results. For, if Nazism "bedevils" Gregor's analysis, too many other movements do not. If one interprets fascism as a mass-mobilizing, developmental dictatorship in modernizing nations, fascists exist in abundance, Left and Right, across the world. If Mussolini's Nazi ally does not qualify, what of his Communist enemy, Stalin? And what of the regimes of Castro, Ho, Nkrumah, and Nasser? Gregor has come to accept them all as "fascist" or "fascistic." He admitted that this assumption would probably "outrage" most historians.²⁴ That conclusion was well founded.

Gregor's work appears to redeem Mussolini's prophecy that the twentieth century would be the century of fascism. Fascists, it seems, are everywhere in the Third World; but Hitler remains in a world of his own. In his latest book, however, Gregor speculated on the future prospect of mass-mobilizing movements arising as well in advanced modern economies where the complex problems of technology, environmental pollution, and population growth have resulted in crisis and breakdown. The probable result, he believed, would be a totalitarian, fascist state, an "analogue" of National Socialism in industrial Germany, which would maintain social order during the advance into a "technotronic postindustrial society."²⁵ Although failing to explain the significance of Nazism in its own time, this view did give fascism a potential place in the higher stages of the modernization process in advanced industrial nations. Gregor was too perceptive, however, not to recognize the con-

²¹ Gregor, *Interpretations of Fascism* (Morristown, N.J., 1974), 5, 252.

²² See his *The Ideology of Fascism*, xiv, and *Contemporary Radical Ideologies: Totalitarian Thought in the Twentieth Century* (New York, 1968), 171.

²³ Gregor, *The Fascist Persuasion in Radical Politics* (Princeton, 1974), 413-14.

²⁴ Gregor, "Fascism and Modernization: Some Addenda," 384, n. 50.

²⁵ Gregor, *The Fascist Persuasion in Radical Politics*, 413-20.

sequence of such notions: if fascism appears at different levels of industrial development, the relationship between fascism and modernization thereby becomes loose and arbitrary.²⁶ Stated more abruptly: if fascism can be connected to different stages of the modernization process, it can just as well be disconnected from the modernization process altogether.

In truth, Gregor's recent work reveals in an unconscious way a crisis of confidence among those in fascism studies who placed their hopes in modernization theory. He believed, however, that researchers must go on, bringing to bear all the rigor, interdisciplinary enterprise, and tolerance for novelty that are the special attributes of social science. "The study of Fascism," he concluded, "and of generic fascism, requires nothing less."²⁷ But is there reason to believe that the result will be anything more? Whatever one may think of modernization theory in general (and of late it is thought of less and less), it is wanting in the case of fascism in particular. The time has come to separate the study of fascism not only from modernization theory but from the German and Italian models that have always been used to impose such conceptions upon movements in other countries.

The result, in part, would make the subject less pivotal and less passionate. Fascism would no longer be a political success story between the two world wars. In Europe, the vaunted "era of fascism" was really a tale of two cities, Rome and Berlin. Reputed fascist movements elsewhere, though noisy and extravagant, remained more or less weak and struggling. In Spain, often considered the third fascist homeland, the Falange (numbering perhaps twenty-five thousand members) received only forty-four thousand ballots in the last pre-Franco election of 1936, a figure representing only 0.7 percent of the total national vote.²⁸ The most successful of the lot, the Arrow Cross movement in Hungary, polled about 20 percent of the vote in 1939. In 1936 the combined vote for the two parties in Belgium reached about the same figure. In Rumania the Iron Guard peaked at under 16 percent in 1937. Elsewhere, even in the best years, the numbers trailed downward into insignificance. Although "fascist" influences in these countries no doubt extended considerably beyond the number of voters who supported the movements at the ballot box (a number of organizations, of course, did not participate in political elections), virtually every study indicates that popular support—where it can be estimated—was generally limited and fluctuating. By 1937–38 most organizations were foundering, and even in Italy the younger generation was debating the failure of fascism.

Losing momentum domestically, weighted by the routine of power, and faced abroad with an antifascism growing as fierce as themselves, the regimes in Rome and Berlin followed the historic road of dictatorships confronted with the prospect of diminishing returns at home, the road toward war and

²⁶ Gregor, *Interpretations of Fascism*, 210.

²⁷ *Ibid.*, 262.

²⁸ Stanley G. Payne, "Spanish Fascism in Comparative Perspective," *Iberian Studies*, 2 (1973): 4. The best Nazi vote in a free election was 37.4 percent at the depth of the depression in July 1932.

conquest. "Rather than a 'fascist era,' " Stanley G. Payne rightly concluded, "there was a ten year period of German Nazi hegemony in Europe based above all on military power."²⁹ Europe probably could have lived with Italian fascism. Before 1936 Mussolini's state was, after all, an accepted partner in the Continental balance of power. But no one could live with Hitler, not even supposed fascists in the lands he conquered. Under the new order, the proclaimed fascist organizations in the occupied countries usually ceased to be native movements with historic roots in their own homelands and became instead pale images of their Nazi overlords. In this sense, when European fascism began at last to resemble the kind of generic phenomenon that social scientists seek to define, its history was over as an independent, spontaneous force in the life of the Continent.

Although these movements ended as pro-German parties in their own countries, they did not begin in this way. Nothing does more to demonize European "fascists" than to make them all Nazis from birth. Certainly we must never forget that between 1942 and 1945 such groups from different nations of occupied Europe became involved in the death machinery of the Final Solution. Auschwitz is not solely a German problem. But we should not look at the previous history of these groups through the barbed wire of the concentration camps. As long as the fascism problem is the Nazism problem we cannot separate it from visions of the Final Solution; for this reason discourse on the subject will remain charged, moralistic, and pulpitarian. I will discuss below the work of those historians who interpret Hitler's movement as a unique product of German history. To accept their view is to recognize that other movements also may be unique and that they can only be understood in terms of their own national histories. To implant Nazism exclusively in German history is to dismantle it as a conceptual model. The drama of the Third Reich made popular the impression of Nazism as fascism pure and unchained, a movement with the power and resources to do what all fascists in their hearts wanted to do. The time has come to recognize, on the contrary, that the Hitler regime involves problems too aberrant and peculiar to provide us with conclusions for interpreting movements in other nations. Hard cases make bad law.

Since fascism is ostensibly an international movement, some scholars insist that it can only be understood in terms of an international model, a construct derived from the comparative study of a cross-section of national forms and not from one or two single cases. Moreover, the model ought to be sufficient unto itself, with only minor differences between existing units.³⁰ European historians claim to find abundant national forms of fascism in areas beyond Italy and Germany, but what do these forms share that is cross-national? Beyond Europe the problem gets worse. When, for example, Jordi Solé-Tura affirmed that "no fascist movement recommends the abolition of private

²⁹ Payne, "Spanish Fascism in Comparative Perspective," 146.

³⁰ For this point, see George Macklin Wilson, "A New Look at the Problem of Japanese Fascism," *Comparative Studies in Society and History*, 10 (1968): 407.

ownership of the means of production,” Lloyd Eastman soon found one that did, the Blue Shirts of Kuomintang China: “This does not,” he suggested, “prove that the Blue Shirts were not fascist. On the contrary, it demonstrates again the multifarious forms that fascism assumed in different national settings.”³¹

This is the logic of the cancer cell, and with it there can be no end to the number of fascist movements. Without conceptual boundaries, there are no limits to growth; where such boundaries are imposed, the distinctive elements of the various “fascist” organizations break through the lines at every point. This is not to say that these organizations may not share certain correspondences and similarities.³² Certainly, individual scholars will want to continue the effort to catalogue more effectively a “fascist minimum,” a certified cluster of shared traits (salutes, shirts, squads, *Führerprinzip*, and whatever) that could provide some instant identity to those throngs of nationalist radicals that I uncomfortably call “fascist.” Frankly, I anticipate the list will be short and inconsequential. Such traits are largely descriptive accessories, features too limited and external to provide a compelling generic classification. The so-called fascist parties are too mixed, diverse, and exceptional to be collected into such a general typology. It is not enough, therefore, to replace the German and Italian prototypes with a comparative international model. Instead, it is necessary to declassify fascism altogether as a generic concept.

SECONDLY, FASCISM IS NOT AN IDEOLOGY. To contemporary observers, “fascist” ideas appeared somehow incommensurate with the spirit and spontaneity of fascist action. So suddenly had fascism broken upon Europe, so extraordinary was its appearance, and so shocking its deeds that scholars at first resisted the conclusion that its essence could be contained in a system of ideas. One could demonstrate, for instance, that virtually every “fascist” idea dated back at least to the nineteenth century or, in some cases, to the origins of political thought itself. Fascism, seemingly, must derive from more explosive material. A force so cataclysmic and unforeseen could only have been generated from the catastrophies of our own century: the First World War, Bolshevism, and the Great Depression.

³¹ Eastman, “Fascists in Kuomintang China: The Blue Shirts,” *China Quarterly*, 49 (1972): 29. For the comments of Solé-Tura, see his “The Political ‘Instrumentality’ of Fascism,” in Woolf, *The Nature of Fascism*, 44.

³² The belief in comparable features connecting European fascisms is shared by the two leading authorities on Nazism and Italian fascism in Germany and Italy respectively: Karl Dietrich Bracher and Renzo De Felice. In their works, the two movements each appear as a special case, inherently German or Italian; yet both scholars have suggested the existence of a “minimum common denominator” among the various national forms of European fascism. Their tentative references to these common properties, it must be said, contrast with their forceful demonstration of the uniqueness of the organizations they study and with their insistence on the virtual impossibility of comparing even these two classic fascisms, one to another. For Bracher, Nazism was “even less an export item than Italian Fascism”; Bracher, *The German Dictatorship: The Origins, Structures, and Effects of National Socialism*, trans. Jean Steinberg (New York, 1970), 46. Further, De Felice has professed difficulty in uniting the two movements not only within the same concept but within the same discussion: “They are two worlds, two traditions, two histories”; see De Felice, *Fascism: An Informal Introduction to Its Theory and Practice* (New Brunswick, N.J., 1976), 40, and *Interpretations of Fascism*, trans. Brenda Everett (Cambridge, Mass., 1977), 169–70.

More recently, however, this “big bang” theory of fascism has given way to the arguments of a number of intellectual historians that the real substance of fascism was to be found in its intellectual content. Some in particular have claimed to discover the origins of fascist thought in the intellectual revolt against liberalism and Marxism between 1870 and 1914, when a collection of European thinkers, obsessed by a fear of mass democracy and moral decadence, developed a philosophy of will, emotion, soil, and blood. Fascism, in this way, acquired a history, a connection with what are sometimes described as “prefascist” ideas and thinkers existing before the movement itself. These historians attempt not only to identify a body of ideas, an “ideology” of fascism conceived as more or less analogous to the conventional ideologies of liberalism or socialism, but also to comprehend its psychological pull, to “understand” fascism by means of an intuitive grasp upon the consciousness of its adherents. They seek to understand fascism by understanding its appeal. In extending its intellectual origins into the nineteenth century, they seek the reasons why men came to the movement itself in the twentieth century. George Mosse, for instance, admitted that the mental shock of the First World War was necessary to provide the popular base of fascism but insisted nevertheless that the thought of the nineteenth century conditioned this response in the European mind.³³

The “big bang,” it seems, was ignited by a long fuse. Most scholarly efforts to trace the ideas involved have been exercises in good old intellectual history. In raw form, the method proceeds as follows: first the researcher isolates an idea in the thought of a reputed fascist ideologue; next he finds an earlier thinker who appears to have originated, possessed, or transmitted the idea; and, by connecting one with the other, he assigns to the earlier thinker a place in the philosophical tradition leading to fascism. This “precursor” is then often dubbed a “prefascist” or “protofascist.” Edward R. Tannenbaum recently presented a terse critique of such procedures: “The observation that two successive things are similar does not prove that the later one has a direct connection with the earlier one. To assert such a connection without empirical proof is a logical fallacy: *post hoc, ergo propter hoc*. And to use the similarities as evidence is to argue in a circle.”³⁴

The same objections, of course, can be raised against the attempt to trace the lineage of any ideological system. The transmission of thought is inherently an obscure and subterranean process. Even the classic ideology of liberalism is ragged with disconnected ideas, and socialism is as disordered as its origins in the contrasting thought of Karl Marx, Claude Henri de Saint-Simon, and Robert Owen. If the problems are the same in the case of fascism, the confusion is worse. Here historians disagree not only over who the intellectual forebears were, or whether there were any forebears at all, but also whether fascism itself is an ideology in the first place. If one grants that it

³³ Mosse, “Fascism and the Intellectuals,” in Woolf, *The Nature of Fascism*, 246.

³⁴ See Tannenbaum’s joint review of Zeev Sternhell, *Maurice Barrès et le nationalisme français* (1972) and of Robert Soucy, *Fascism in France: The Case of Maurice Barrès* (1972), in *AHR*, 78 (1973): 1478–80.

is, the next question is why virtually the same ideas should be termed "prefascist" in one century and "fascist" in another.

More importantly, it is improbable that the nature of fascism can be represented by means of selected quotes from intellectual works. This interpretation of fascism—as an ideology derived from philosophic and literary influences in the last century—stems largely from studies of a limited number of "classic" cases, intellectuals whose writings and thought offer the most compelling and dramatic expression of what particular investigators conceive to be the essence of fascism. In the case of France, for example, the writings of the self-proclaimed fascist novelists Pierre Drieu la Rochelle and Robert Brasillach portray clearly their own perception of the movement as a spiritual revolt against decadence and moral decline. As a result, their books and ideas are the primary source of evidence for historians who themselves perceive fascism in the same way.³⁵ Indeed, Drieu la Rochelle's talent for dramatizing in haunted prose his own virtual obsession with decadence has made him the *beau idéal* of intellectual historians of fascism. He is, George Mosse commented, "the most interesting French fascist."³⁶ One may wonder, however, whether Drieu la Rochelle interests scholars of today more than he interested the fascists of his own time. How much can such intellectuals instruct us about the movements of which they were a part? As a propagandist for Jacques Doriot's *Parti populaire français*, Drieu la Rochelle had little influence in the ideological debates within the organization, and certainly his ideas made no impression on Doriot himself.³⁷ To show that intellectuals like him were influenced by "prefascist" forebears is one thing; to show that they themselves influenced other fascists in their turn is something else altogether.

In the study of political movements, of course, one must always distinguish between men of ideas and men of action, between intellectuals concerned with the integrity of ideological commitment and leaders concerned with the practical problems of exercising power. Beyond this, the relationship between fascist ideas and fascist leaders presents a particular problem of its own. Although politicians throughout history have not always told the truth, Mussolini and Hitler were the first to make a public creed of lying. The practice confounded their enemies and historians alike. Such men simply cannot be taken at their word, yet the quoted word is the mode of intellectual history. Confronting confessed liars compromises the traditional method of using quotations from speeches and writings to document arguments on the belief or motives of historical personalities. Historians, of course, are trained to distrust all human testimony. In dealing with testimony from the likes of Hitler and Mussolini, however, the safest course is to discount everything. In

³⁵ See, for example, Robert Soucy, *Fascism in France: The Case of Maurice Barrès* (Berkeley and Los Angeles, 1972), and "The Nature of Fascism in France," *Journal of Contemporary History*, 1 (1966): 27–55; and William R. Tucker, *The Fascist Ego: A Political Biography of Robert Brasillach* (Berkeley and Los Angeles, 1975).

³⁶ Mosse, "Fascism and the Intellectuals," 214.

³⁷ See my "Jacques Doriot et l'esprit fasciste en France," *Revue d'histoire de la 2^{ème} guerre mondiale*, 97 (1975): 31–44.

public discourse, both were notorious for working both sides of the street. Hitler's strategy of the big lie is best explained by the man himself. Mussolini described his own method as the technique of the "Scotch douche," gushing alternately hot and cold, radical and conservative, sounding reasonable at one moment and intransigent at the next, whatever the occasion demanded.³⁸ The question, therefore, is not whether these leaders believed in certain ideas but whether we could believe them even if they said they did.

Aside from this problem of historical method, there exists the larger problem of the content of fascist ideology itself. It should be remembered that the concept of international fascism arose out of the political battles of the 1930s and that the debate on its idea system was first shaped by a generation of scholars who reached maturity during the same period. The result has been the academic creation of fascist ideologies that are far more consistent and universal than anything created by the fascists themselves. European fascists were never successful in defining a "unifascist" ideology. The Italian experience demonstrates this best, for the meaning of fascism was as uncertain to the first fascists as it is to us.

In the beginning, Mussolini reportedly considered fascism to be strictly "our thing," a product unique to Italian genius and temperament, and clearly marked "not for export." Little definition, therefore, was required—nor were all fascists sure there was one. "I am fully aware," admitted one of the Duce's ideologues in 1925, "that the value of Fascism as an intellectual movement baffles the minds of many of its followers and supporters and is denied outright by its enemies."³⁹ In his informative study, *Universal Fascism*, Michael Ledeen noted that, since the doctrine was so vaguely defined, Blackshirt lieutenants were confused about how to distinguish between heresy and conformity in Italian thinking. With the rise of Hitler, however, came a "profound change." Rome was confronted with a foreign "fascism" that was, at the same time, an ideological rival in the struggle for influence over fledgling movements emerging in other countries. At stake, Mussolini was told by his propagandists, was his place in history as the creator of fascism. The challenge he faced was the problem of fascism itself, the problem of giving transcendent meaning to an Italian expression, of finding an international definition for a phenomenon previously defined in national terms. In addition, there was the problem of defining it against Nazism, a force reviled at Rome as pagan, anti-Semitic, and alien to the fascism originated by Italians. "Fascist after fascist," according to Ledeen, "wrote of the folly of racist doctrine, stressed the humanistic and religious components of Italian fascism, and attacked Hitler."⁴⁰ In his famous encyclopedia article in 1932 and through his representatives two years later at the international fascist congress at Montreux in Switzerland, the Duce attempted to define "universal fascism," to

³⁸ Denis Mack Smith, *Mussolini's Roman Empire* (New York, 1976), 11.

³⁹ Alfredo Rocco, "The Political Doctrine of Fascism," in Carnegie Endowment for International Peace, *International Conciliation*, no. 223 (1926): 408.

⁴⁰ Ledeen, *Universal Fascism* (New York, 1972), 101.

pre-empt the role of its prophet, and to establish Italy as its spiritual—and financial—source. But this effort to bring meaning and order to fascism was no more successful than the others.

The Montreux Congress, called to establish the principles of fascist unity, divided instead on the Jewish question. Before departing, the delegates managed to agree on common articles of faith in the monolithic state, economic corporatism, something called the “national revolution,” and, above all else, the proposition that each nation must solve its problems in its own way. This creed was so general, concluded Ledeen, that most elements of the extreme Right in Europe could have agreed to it as well, including traditional corporatist groups within the Catholic Church. Disappointed, the Italians thereafter let this first project for universal fascism lie dormant. In 1936, however, diplomatic events brought Hitler and Mussolini into a marriage of convenience that soon generated the need for a matching ideology. The Axis alliance, historians now recognize, was based on a personal relationship between two very different men, companions without communication, each needing the cooperation of the other and each resigned as a result to tolerating the inflexible opinions that divided them.⁴¹ The dictators were more interested in minimizing their ideological differences than in debating them. More important were things that could bring them together, ideas and enemies that would give “fascist” meaning to a politicomilitary association between two grasping partners. Here Nazism, obsessed from the beginning with Germanic notions of a world-historic mission, offered more possibilities than the opportunistic creed of the Blackshirts. Thus, Mussolini incorporated into a revised version of universal fascism racial politics and the “crusade against Bolshevism,” issues previously of little importance at Rome.⁴² To this, the antifascists of Europe added dimensions of their own, and the revised version became the standard version, the popular conception of the ideology of fascism. “During the thirties . . .,” recalled Stephen Spender, “Fascism meant dictatorship, censorship, the persecution of the Jews, the destruction of intellectual freedom. To be anti-Fascist was to be on the side of humanity. Conversely, to be Fascist meant to be against it.”⁴³

In the 1950s a school of Western scholars elevated this propagandistic vision of fascism, formed primarily from Nazi episodes, into the political science concept of totalitarianism. There, in the work of Hannah Arendt, fascist ideology took its place beside Marxist ideology as the driving force in a new system of government that sought to destroy the idea and existence of free individuals.⁴⁴ Through an escalation of ideological concepts fascism had become identified with Nazism, Nazism with totalitarianism, and totalitarianism with terror and concentration camps. In the early 1960s, however,

⁴¹ F. W. Deakin, *The Brutal Friendship: Mussolini, Hitler, and the Fall of Italian Fascism* (New York, 1962); and Mack Smith, *Mussolini's Roman Empire*, 53–54, 129–30, 142–43.

⁴² Informative here is the neglected article of Phillip Cannistraro and Edward Wynot; see their “On the Dynamics of Anticommunism as a Function of Fascist Foreign Policy,” *Politico: Revista de Scienze Politiche*, 38 (1973): 645–81.

⁴³ See Spender's foreword to Alastair Hamilton, *The Appeal of Fascism* (New York, 1971), x.

⁴⁴ Arendt, *The Origins of Totalitarianism* (New York, 1951).

the work of two men, Ernst Nolte and Eugen Weber, broke with old stereotypes and inspired a revival of fascist studies that continues to the present day.

Nolte's contribution was a work difficult, brilliant, and exciting. It was also a mystification. What Arendt did to turn fascism into political philosophy, Nolte did to turn it into metaphysics. He ascribed its source within men to a condition of consciousness that he defined as "resistance to transcendence," a ponderosity explained most simply as a fear of modernism's power to disintegrate nations, races, and cultures.⁴⁵ As one critic noted, this interpretation was hard to apply to Mussolini's Blackshirts, the movement that gave fascism its name in the first place.⁴⁶ Arguments for a universal fascist ideology seem in fact to depend upon ascending, in the manner of Arendt and Nolte, to the undemonstrable realm of metaphysics. At lower levels, Italian fascism and German National Socialism fit as badly into a common idea system as they do into a common stage of the modernization process.

The value of Weber's work, as I remarked above, lay in his decision to separate the two movements. Nazism, he believed, was motivated by radical doctrine, Italian fascism by a more practical pursuit of action and power. The result of this logic was not only the de-Nazification of Mussolini's party, but the "doctrinalization" of Hitler's; the movement of the "big lie" apparently believed in certain truths as well. "It is quite obvious," Weber wrote, "that the Nazis of all people were ready to make the most extraordinary sacrifices for the sake of their theories and twisted ideals."⁴⁷ On reflection, ideological belief indeed appears to have been necessary to Nazism in a way that it was not to Italian fascism—Auschwitz cannot be explained as opportunism or demagoguery. The Final Solution would appear to undermine all theories that fascism was mere phrasemongering and mass deception, a force without convictions or ideology. Logic demands that such sustained and systematic horror, pursued so relentlessly in the face of approaching defeat, must have resulted from the actions of fanatical men committed to fanatical ideas. If Auschwitz is the riddle of fascism studies, it also appears to be compelling evidence for the arguments of intellectual historians that at least German fascism was a movement of ideas and that ideas have consequences.

This is what gives particular credence to the work of those scholars who seek the ideological roots of Nazism in the unique *völkisch* traditions of the German past. What made German fascism a thing apart, contended George Mosse, was a distinctive mood and depth of feeling inherited from *völkisch* culture, a concept of the world and man that penetrated into the national consciousness of the German people.⁴⁸ "It is therefore dangerous," he asserted, "to extend the ideological foundations of the German fascist experience to other countries."⁴⁹ The continuing debate on the historical connection

⁴⁵ Nolte, *Three Faces of Fascism*. This work was first published in Germany as *Der Faschismus in seiner Epoche* (Munich, 1963).

⁴⁶ Klaus Epstein, "A New Study of Fascism," *World Politics*, 16 (1964): 320.

⁴⁷ Weber, *Varieties of Fascism*, 142.

⁴⁸ Mosse, *The Crisis of German Ideology: Intellectual Origins of the Third Reich* (New York, 1964).

⁴⁹ Mosse, "Fascism and the Intellectuals," 217. Mosse first argued for the difference between National Socialism and Italian fascism in the early 1960s; see his *The Culture of Western Europe* (Chicago, 1961), 341–75.

between the *völkisch* tradition and National Socialism will not be discussed here. To demonstrate, however, that Germanic ideas prepared the way for Nazism is not to demonstrate that the Nazis themselves believed in them. Indeed, Fritz Stern concluded that such ideas probably mattered for naught to these vulgar men, “who held thought itself in contempt and regarded ideas as mere weapons in the political fight for power.”⁵⁰ Thus, neither the link between *völkisch* ideas and the Nazi mind nor that between ideology and Auschwitz is closed. I observed above that logic appears to demand an ideology behind the events of Auschwitz. If none existed, it would not be the first time that the death camp has confounded human reason. In sum, the question is still open whether the Final Solution is a study in an ideology of evil or, as Arendt argued in the case of Adolf Eichmann, in the “banality of evil.”⁵¹

In the search for a universal fascist ideology, the debate on *völkisch* ideas is, in any case, a dead end. Everything stops at the German and, perhaps, the Austrian boundaries. What emerge more clearly, however, are the inherent ideological differences—whether the ideas are believed or not—between Nazism and Italian fascism. In interpreting Nazism as a special case, a number of historians have recognized this need to explain European fascism in at least two ideological forms. What has become somewhat common in their work is a rough geographical division of fascism into ideological spheres: (1) in the Mediterranean countries, they contend, movements arose that were inspired by the “original” fascism of the Blackshirts—movements carrying an ideology of activism, corporatism, and integral nationalism; (2) in Germany and, to an extent, in Austria, a different and singular movement appeared—a fascism of race and soil, steeped in a peculiar *völkisch* mythology; and (3) elsewhere, variants of these two primary forms emerged—different national mixes of ideas flowing from the founts at Rome and Berlin.⁵²

As with most things in the study of fascism, the German and Italian movements have supplied the main evidence for interpretations of fascist ideas. Unfortunately, we will be no more successful in comprehending other groups in terms of these two ideological forms than we have been in the past when attempting to comprehend them in terms of one form. It is probably true that every proclaimed fascist organization drew inspiration from both the German and Italian parties and some imitation was therefore inevitable, but most such organizations had the ambition to create their own national versions of the same thing, movements expressing the uniqueness of their own national character and traditions. When Mussolini invited the leader of the *Falange Española*, José Antonio Primo de Rivera, to attend the international fascist congress at Montreux in 1934, he flatly refused. The Falange was not fascist, he protested, it was Spanish.⁵³

⁵⁰ Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley and Los Angeles, 1961), 294.

⁵¹ Arendt, *Eichmann in Jerusalem: A Study in the Banality of Evil* (New York, 1964).

⁵² Wolfgang Sauer, “National Socialism: Totalitarianism or Fascism,” *AHR*, 73 (1967): 421; and Charles F. Delzell, *Mediterranean Fascism, 1919–1945* (New York, 1970).

⁵³ For his press release, see Delzell, *Mediterranean Fascism*, 278.

The time has come for historians to admit what Mussolini himself was forced to recognize: universal fascism is an illusion. Whether in one form or another, fascism was not an ideology in the style of the great nineteenth-century ideologies, that is, a thought system that provided a theoretical outlook upon experience. Some observers have remarked that this very absence of ideology was what doomed fascism to virtual extinction after 1945. It had nothing for men to carry forward, no key to knowledge, no view of history, no ideal for the future. Nor was it connected to anything permanent in society. It represented the outlook of neither a social class, an economic interest, nor a social organization.⁵⁴ The movements we call fascist were not historical accidents. They were conceived and nurtured in the womb of society. Yet, when they appeared, they were also a kind of "happening," a collection of forces in motion, at once spontaneous and imitative, committed and unprincipled, extroverted and self-absorbed. I remarked earlier that there is no such thing as fascism *per se*; there are only the men and organizations that carry that name. When they were defeated, or when their moment was over, fascism passed into history with them.

FINALLY, FASCISM IS NOT A PERSONALITY TYPE. "Things fall apart; the centre cannot hold. . . . The best lack all conviction, while the worst are full of passionate intensity." So wrote William Butler Yeats in 1920 in a poem that served to foretell the politics of the 1930s, the eager activity of fascists and the passive resignation of their opponents. Indeed, when contrasted with the barrenness of fascist programs, the fervor and intensity of so-called fascist behavior caused many scholars from the start to seek the roots of the phenomenon in psychological drives. In the absence of ideology, fascism was made an emotion. While intellectual historians were attempting to connect fascists to a set of common ideas, investigators of this persuasion attempted to connect them to a common personality structure.

Their method is frequently that employed by psychohistorians, and the familiar landmark is Erich Fromm's *Escape from Freedom*, the study of fascist man as a sadomasochistic personality. I will not repeat the conventional criticisms of this still-engrossing work⁵⁵ but will observe only that such interpretations make fascism both mystical and internal, associating it with irrationality and dark urges, and locating its source in those areas of the personality most inaccessible to empirical analysis. To an extent, of course, all political behavior has roots in the unconscious. Nor do I wish to deny that, for individuals of certain mentalities, fascist movements probably possessed an attraction that they did not possess for others. Rather I deny the idea that such a mentality *is* fascism, a notion that is but the final step in reductive logic. Fascism is not a mental category, a complex of emotions and irrationalities existing in the mind. Too many of our illusions have been preserved by

⁵⁴ Eugen Weber, "France," in Hans Rogger and Eugen Weber, eds., *The European Right* (Berkeley and Los Angeles, 1965), 125.

⁵⁵ For the latest critique, see Gregor, *Interpretations of Fascism*, 64-73.

this method of internalizing a concept that cannot be explained satisfactorily from evidence in the external world. And too much that does not make sense in our theories of fascism has been concealed by an excessive emphasis on its irrationality.

Psychologists and psychohistorians have not been the only ones to take refuge in this reduction of fascism to mental processes. The more other theories dissolve under objective analysis, the more traditional historians are tempted to venture into the realm of psychological explanations. In preparing their remarkable anthology, *The European Right*, Hans Rogger and Eugen Weber asked a number of Western scholars to contribute essays on the rise of what they described as "the new Right," a radical, antiliberal phenomenon, which, they believed, was distinct from traditional authoritarianism and reaction. In practice, the assignment was to trace the development of fascism out of the intellectual environment of eleven individual European countries. In the concluding essay of the finished work, Rogger acknowledged that the comparison mainly revealed a lack of things: lack of uniformity, lack of shared doctrine, lack of a uniform class base, and, in truth, lack of intelligence. Yet, in the end, he claimed to discover in fascist minds the common streak that was missing in everything else. "In the final reckoning . . .," he wrote, "the Right represents . . . a nihilistic hostility to modernity, a fear of the unfamiliar, and an infantile yearning for protection . . . against dark and only dimly comprehended forces that lurk and threaten on all sides."⁵⁶

We are aware already of the problems involved in interpreting fascism as a form of antimodernism. Such problems are only compounded by making it an expression of "the new Right" as well. That analysis has, for instance, little relevance to an important element present in virtually every reputed fascist organization—those men who came from the Left. If ideas mattered at all in their political development, they were ideas considerably different from those stressed in these studies. If Hitler was formed by influences on the Right, the ex-Communist Jacques Doriot was formed by Marx and Lenin, the ex-socialist Marcel Déat in part by Henri de Man, and the old Labour MP Oswald Mosley by John Maynard Keynes. These men are hard to place in the political categories that Rogger has described as the new Right. More to the point, it is impossible to imagine them within the psychological categories that he believed underlay this phenomenon itself. I suspect that these hard and calculating opportunists had a grip on reality at least as rational as that of anyone who would interpret them as "infantile" antimodernists, yearning for protection against things that go bump in the night.

Nevertheless, I can understand the temptation to locate the substance of fascism within the personalities of such men. Some years ago I traced the tortured journey of Jacques Doriot from communism to fascism. What I described then as the "desperate logic" behind his conversion, the evolving circumstances of his vendetta against his old Communist comrades, appeared to me to be plausible, honest, and straightforward. But it left me feeling

⁵⁶ Hans Rogger, "Afterthoughts," in Rogger and Weber, *The European Right*, 587–88.

vulnerable. Compared to the descriptions of fascism found in other writings, Doriot's version seemed pale and deprived. He was neither a racist nor an elitist, nor was he a militarist; the reputed fascist mysteries of intuition and irrationalism interested him not at all, and even his nationalism was sparing and somewhat contrived. Other historians made fascism of sterner stuff. Wary, feeling defensive, I decided to take the plunge. "Doriot always had an inclination toward action and movement that seemed innate rather than acquired," I argued, and I went on to root fascism in the urges of his personality.⁵⁷ Having failed to connect his ideas with the conventional themes of fascist ideology, I connected his emotions with the conventional themes of fascist man. *Mea culpa*. My transgression was not to misrepresent Doriot's personality but to misrepresent fascism: to submit to the tyranny of a concept, to accept fascism as an absolute, to "objectify" it, and to make it into a thing. Rather than learning from real life, I fell back upon an illusion. As a result, I obscured the true lessons of the experience of Doriot and his party: to wit, the independent quality of his "fascism" as it emerged within the French national community and, thus, the impossibility of comprehending it through generic concepts or ideological abstractions.

"FASCISM OFFERS AN IDEAL MEETING GROUND for historians, political scientists, sociologists and economists," S. J. Woolf suggested in 1967. "It enables them to refine the precision of their methodological tools and test the validity of their hypotheses."⁵⁸ In reality, however, social scientists go away from most conferences on the subject more confused than when they arrived. Stanley G. Payne has concisely described the reason: "The term fascism can be applied to the entire broad genus only at the cost of depriving it of any specific content."⁵⁹ In this connection, I can summarize somewhat ungrammatically the conclusions to the present article: the concept of fascism should be demodeled, de-ideologized, de-mystified, and, above all, de-escalated. It will not be the first time that an "ism" has been discovered to embrace things unique and too diverse to be defined in terms of general categories or specific ideas. "We came to believe that this word 'romanticism' was only a word," wrote the French poet Alfred de Musset in 1836. "We found it to be beautiful, and it seemed unfortunate that it meant nothing." Yet the word romanticism has been retained to refer to a movement of men and ideas in a particular historical epoch. They are men and ideas that we have been conditioned, perhaps without sufficient reason, to associate together but that most of us acknowledge are recognizably distinct; and they will probably always elude collective definition. The word fascism deserves a similar fate.

Because fascism is "dead," seemingly swallowed up and consumed by the Second World War, Ernst Nolte has reasoned that we can set limits to its

⁵⁷ Allardyce, "The Political Transition of Jacques Doriot," *Journal of Contemporary History*, 1 (1966): 72. For my attempt to compensate for this problem, see "Jacques Doriot et l'esprit fasciste en France."

⁵⁸ Woolf, Introduction to his *The Nature of Fascism*, 4-5.

⁵⁹ Payne, "Spanish Fascism in Comparative Perspective," 142.

history and designate the period 1919–45 as the “era of fascism.” The memory of the Final Solution will, most likely, always keep fascism immediate and alive for scholars of the human sciences, and no doubt resistance will continue against permitting the record of its deeds to become “only history.” There is, nevertheless, something to recommend the idea of confining the term fascism within the time limits that Nolte defined. Full of emotion and empty of real meaning, the word fascism is one of the most abused and abusive in our political vocabulary. Unlike the word romanticism it is not found to be beautiful. But it is similar in that it means virtually nothing. Yet the term fascism is probably with us for good. The object, therefore, is to limit the damage. Placing it within historical boundaries at least provides a measure of control, restricting the proliferation of the word in all directions, past and present, and preventing it from distorting political rhetoric in our own time. Fascism must become a foreign word again, untranslatable outside of a limited period in history.

I have argued here that fascism must become recognized as merely a word within this limited period as well, undefinable beyond the individuals and organizations that it is used to identify. No doubt those who believe that further research will ultimately provide a universal definition of fascism are likely to interpret this article as a counsel of despair. Where they are concerned, it is meant to be a counsel of despair. The search for the meaning of fascism has yielded few convincing results to those social scientists concerned with forming general theories on human experience. For historians, the best advice is not to despair over the present direction of research but to follow it. Research is leading toward the disintegration of what remains of “unifascism” as a generic or ideological concept and the replacement of it with a fascism at once more simple and more difficult: more simple in that the term is becoming disentangled from universal abstractions, more difficult in that it covers a bewildering variety of political expressions. The task, therefore, is to study these political expressions just as they are, while recognizing that the name given them is less intelligible than we would like it to be. Anyway, there is no perfect history any more than there is a real fascism.

Comments:

GILBERT ALLARDYCE'S ESSAY IS A WELCOME DEFLATION of the excesses and reification frequently encountered in theorizing about "fascism." Most of the keenest students of the major, putatively fascist movements or regimes have become extremely uncomfortable with the airy and unempirical generalizations commonly bandied about as either definitions or interpretations of fascism. Closer examination almost always reveals that these generalizations do not apply to many, or even most, of the fascist movements—and sometimes to none at all. The problem has become particularly acute in Germany, where writers and scholars (commonly of Marxian, or what passes for Marxian, inspiration) generate firm abstractions about "fascism," chiefly on the basis of the German experience.¹ Since they frequently avoid empirical analysis almost altogether, the problem has often degenerated into a purely semantic debate about labels.² In general, therefore, I emphatically agree that what is referred to as European fascism cannot be reduced to an exact generic concept of uniform content, to a common ideology, or to some sort of unique personality type. As Professor Allardyce has pointed out, I have elsewhere indicated my disagreement with any unifascist theory.

Yet one question remains: is *any* comparative definition of "fascism" feasible—if we grant that we are not dealing with a unified generic concept—or should the term be avoided as a political category in any sense? At the conclusion of the section dealing with fascism as a generic concept, Professor Allardyce briefly considers the alternative of a short descriptive comparative typology or "fascist minimum." He concludes that a set of common characteristics may be constructed with a greater or lesser degree of accuracy but doubts the utility even of this. To the question of whether or not the formulation of any sort of "fascist minimum" or pluralist categorization is of any value, however, I would respond with a qualified "yes." Historical understanding requires us to identify certain common features or qualities of new forces within a given period, if only to recognize and clarify their differences and uniqueness.

A slightly different way of restating the dilemma would be to observe that a series of radical nationalist movements with revolutionary aims that were at one and the same time anti-Marxian, antiliberal, and anticonservative (in the conventional political sense) appeared in Europe between the world wars. Do they merit recognition as a category in some cautiously delimited and pluralistic schema for purposes of political analysis and classification? Or is it more

¹ For a solid analysis of some of the main problems involved in the German Marxian interpretations, see A. G. Rabinbach, "Toward a Marxist Theory of Fascism and National Socialism," *New German Critique*, 3 (1974): 127–53.

² Wolfgang Schieder has accentuated this problem; see the introductory remarks and summary to Schieder, ed., *Faschismus als soziale Bewegung* (Hamburg, 1976).

accurate and satisfactory to emphasize their differences and perforce subsume them into some broader category of radical or revolutionary mass movements? This is a most difficult question of historical-political taxonomy; and, though the radically particularistic historian—interested only in the nominalist approach—may respond that for him the question does not apply, those interested in systematic, comparative analysis cannot elude it.

Putative fascists had great difficulty wrestling with this problem in the 1930s and were unable to resolve it satisfactorily even for themselves. Although in 1930 Hitler briefly agreed that National Socialism would bring the “fascistization” of Germany and although Mussolini hailed Hitler’s triumph in 1933 as the victory of “German fascism,”³ during 1934–35 the *fascisti* realized that major, indeed profound, differences existed between the Italian and German movements and broadcast their finding with their customary rhetoric and hyperbole.⁴ Similarly, during the late 1920s and 1930s, the Mussolini regime, because of its “conservative” and “capitalist” cast, alienated the leading figures of various putatively fascist movements. When the Italians tried to identify and develop a sort of fascist International, they proved unable to define adequately either their own ideology or a common set of doctrines. This was due to the great gap between their own theory and practice in Italy and to the absence of any founding creed or sacred writing, as well as to the extreme differences between the approaches of various national groups or their lack of ideological clarity. Doctrines of race and anti-Semitism were a major stumbling block and the only mutual ground erstwhile fascists could find was a common stress on radical nationalism, however variously defined.

The question of generic “fascism” was a major problem for Spanish Falangists, who showed an increasing aversion to the word. Professor Allardyce shows that Doriot’s PPF disavowed the term, as did, I might add, the Belgian Rexists in their early years. Radical, nationalist, multiclass “new parties” tended to move in an increasingly authoritarian direction, but this did not by itself make them necessarily “fascist.” Ultimately, such unity as was achieved stemmed from Nazi influence, which grew with the German imperium. It is not an exaggeration to speak of the Nazification of radical nationalist or fascist movements in Europe after 1937–38. That trend, however, reflected increasing great-power dominance over otherwise multiform political manifestations and did not last long enough or go far enough—owing both to lack of time and to inherent contradictions—to produce anything that could be called a single genus.

THE PROBLEM REMAINS whether it is useful to set the new revolutionary nationalists off in some fashion from other radical or revolutionary groups, such as Communists, socialists, and anarchists on the Left and rightist

³ See Meir Michaelis, “I rapporti tra fascismo e nazismo prima dell’avvento di Hitler al potere (1922–1933),” *Rivista Storica Italiana*, 85 (1973): 544–600.

⁴ For some excellent examples, see Denis Mack Smith’s ridiculing, sometimes misleading *Mussolini’s Roman Empire* (London, 1976), 44–58.

radicals and traditionalist reactionaries on the Right. I believe that, despite all the conceptual and empirical qualifications that must be recognized, the answer is still yes, as long as we recognize that we are dealing with a multiform, hypothetical category and not a unified phenomenon with a common ideology, common structure, common causes, or even common motivations.

Postulation of a carefully delineated fascist ideal type does not require any of the Procrustean fittings or reductionist theories that Professor Allardyce has so effectively criticized. It simply recognizes that the revolutionary nationalists of interwar Europe had certain things in common that set them off from other parties or groups, even though they possessed no absolute common identity among themselves and in fact disagreed profoundly, sometimes violently, about major aspects of policy and doctrine. Such an ideal type or criterial definition need not be called "fascist," save for convenience's sake or out of respect for historical precedent. The typology would serve to distinguish the more revolutionary, authoritarian nationalist groups from the radical right-wing authoritarians, the Hitlers from the Hugenburgs, as it were, and from the more moderate constitutional authoritarians, like the Brünnings and Gil Robleses. Space forbids such an exercise here, and whether or not it is really worthwhile will probably depend on the importance individual scholars attach to general analytical categories. Political scientists studying political history presumably require something of the sort, but particularistic historians, who are given to descriptive kinds of radical nominalism, may find the construct either unnecessary or too abstract and artificial for their individual studies.

Even a multiform typology of fascism would properly refer to movements rather than to regimes. Only two of the putatively fascist movements developed regimes, and they had little in common other than varying degrees of authoritarianism and varying degrees of nationalism. A "single-party-system" typology would cut broadly across all categories of regimes. Indeed, if the choice lies between reified, totally abstract, or narrowly reductionist unifascist theories and no typology at all, the latter is certainly preferable. I do not believe, however, that the alternatives are quite that stark.

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GILBERT ALLARDYCE HAS BROUGHT UP THE HEAVY ARTILLERY to bombard the enemy position: that of generic fascism or, as he calls it, "unifascism." But I fear that the cannon he uses are too large. In the attack on secondary

positions—such as the “bipolar viewpoint”—one wonders, furthermore, whether he shoots with live ammunition or just with blanks.

“Ockham’s razor,” to use Professor Allardyce’s metaphor, cannot be stopped arbitrarily, for it slices off all general concepts by declaring them to be mere *flatus vocis*—or “constructs,” to use the modern expression. But we do not need to fight the controversy between nominalists and realists all over again in order to see that a historical concept is not useless merely because it covers a variety of very different phenomena. “Constitutionalism,” “liberalism,” and “parliamentarianism” are concepts that have had very different meanings in various European countries at different times. To discard these and other concepts for that reason would be to abandon the capacity to order and make comprehensible the great mass of historical facts with which they are concerned. One need not be a “realist” in the medieval sense to regard concepts as something more than mere intellectual constructions. When we speak of “brothers,” we mean a group of men whose resemblance is obviously established by nature itself. But how many differences can be discerned among them at the first close look! Hence, it is often better to differentiate, to speak, for example, of “unitary,” “dualistic,” or “federal constitutionalism.” Nevertheless, there must always be a “central meaning”: all kinds of constitutionalism must be distinguishable from all kinds of absolutism.

Professor Allardyce is doubtless correct in his opinion that the word “fascism” is one of the “most abused and abusive” terms in our political vocabulary. In scholarly usage, however, the term has been given a central significance so general that distinctions are unavoidable and yet so concrete that clear chronological limits for the phenomenon can be established. If I may cite my own work as an example, Hitler’s National Socialism was “radical fascism” and was very different from Mussolini’s “normal fascism.” Yet both were anti-Marxist movements that sought “to destroy the enemy by the evolvment of a radically opposed and yet related ideology and by the use of almost identical and yet typically modified methods, always, however, within the unyielding framework of national self-assertion and autonomy.”¹ In my opinion this definition is valid only for the period between the world wars, the period during which these kinds of movements characteristically appeared, and the period that must, therefore, be described as the “epoch of fascism.” I cannot see that any of the differences cited by Allardyce is so grave and so unnoticed in the discussion up to this point as to require or even make advisable the abandonment of this concept when used with scholarly caution for scholarly purposes.

Professor Allardyce himself cannot do without the concept. At one point he seeks to substitute the expression “throngs of nationalist radicals,” but elsewhere he speaks of the “men who came from the Left” and who had played a role in almost every allegedly fascist organization. In another place he asserts again that Hitler and Mussolini were the first to make lying a public virtue.

¹ Ernst Nolte, *Three Faces of Fascism*, trans. Leila Vennewitz (New York, 1966), 20–21.

“Throngs of nationalist radicals” also existed before the First World War, but they were not as a rule attractive to men of the extreme Left. Nor did any leader of a large party or chief of state in Europe during that period employ the lie as a standard technique of propaganda. Powerful parties and successful regimes of the extreme Right, which attracted numerous and known men of the Left and employed new techniques of propaganda and domination, are so patently different from “throngs of nationalist radicals” that one is compelled to form a new concept, if new wine is not to be poured into old bottles. To summarize: in attacking fascism as a generic concept, Allardyce either strikes merely at the slogan that once played such an important part in the political struggle and has recently reappeared, or he follows too closely the trail of the nominalists, for whom all concepts and, hence, every historical interpretation is a mere “construct” of the intellect (the last sentence of Allardyce’s article actually points in this direction).

Certainly, the common denominator of the movements and regimes belonging to the “fascist type” is difficult to grasp, since not only were all fascist parties embedded in their respective national environments, but, being nationalistic, they definitely wanted to be so embedded. Yet the utterances by Doriot and Mosley, cited by Professor Allardyce, were spoken in a particular context and can be easily matched by other utterances by the same men that acknowledge certain universal values. Fascism begins at the point where nationalism becomes radicalized and, therefore, changed. To contrast the multiplicity of European national fascisms in the era of the world wars with the alleged uniformity of the “Communist world movement” is not very helpful. That uniformity rested on uncontested domination by the Communist Party of the Soviet Union as the only governing Communist Party. After World War II that unity quickly broke apart under the impact of the differences and conflicts between nations and states. At the risk of oversimplification, one could say that the twentieth century is no longer clearly oriented in a national direction, but not yet in an international direction. Fascist as well as Communist parties bear witness to this fundamental fact despite their deep differences.

On some points Professor Allardyce’s criticism is valuable because it reveals how many possible interpretations have been worked out or refurbished by non-Marxists during the last fifteen years. From this point of view, the logical inconsistency of his three principal points (namely, if fascism is not a “generic concept,” there can *eo ipso* be no “fascist ideology” and no “fascist personality type”) is no obstacle. The application of modernization theory can, indeed, lead to variegated results, and it is certainly true that the fascist ideology is not an ideology in the same sense that the great doctrines of the nineteenth century were. I cannot detect where Allardyce’s preferences lie: he appears not to accept A. F. K. Organski’s view of Hitler as “odd man out”; obviously he would like to separate the study of smaller movements that are often called fascistic from the Italian-German model; he is not satisfied with the bipolar pattern of interpretation because the Hitlerian episode is unique; but then he

himself employs the “unifascist” concept by maintaining that the memory of the Final Solution will probably always keep the idea of fascism (!) alive.

THE QUESTIONS PROFESSOR ALLARDYCE RAISES are legitimate and necessary. I would, however, answer them with theses that differ from his:

1. There is a political catchword, “fascism,” which has not been simply fabricated, and which can therefore be transformed into a concept that can be useful to scholars.
2. The concept of fascism is difficult to establish because it relates to a phenomenon that is marked by paradoxes. It was simultaneously national and international, reactionary and revolutionary, bourgeois and populist, modern and antimodern.
3. From the multiplicity of forms comes a multiplicity of interpretations, the consequence of which should be not abandoning of the concept, but differentiating among the forms to arrive at a historical description that is as comprehensive as possible.

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Reply:

SOMEONE HAS SAID THAT DEFINITIONS CONCEAL the wildness of reality behind a wall of words. When that reality concerns men of flesh and blood acting together in collective movements, the words are likely to fail. Erasmus warned that all definitions are difficult; I would add that those words ending in “ism” are the worst. In this connection, Ernst Nolte is correct in criticizing my selective use of Ockham’s razor in the case of fascism. Truly, not only our concepts of fascism could be slashed to pieces by a radical nominalist critique, but also our whole conceptual vocabulary. Have no fear. Concepts are the inevitable inventions of intellectual life, and they will survive as long as scholars reflect on the common nature of individual things; to think is to generate concepts. There is no science here. Scholars, seemingly, have license to define things in any way they please, so long as their explanations are not self-contradictory or otherwise opposed to common logic. Therefore, most concepts will not fit like gloves. What is needed by all is a recognition that such descriptions—like all language—are largely evocative and metaphoric and that a measure of tolerance is generally required, tolerance for exceptions, for variations, and for error. Who among us does not dread the challenge of Voltaire: “If you wish to converse with me, define your terms.” Continually to demand such precision does not enhance communication so much as disrupt it, aggravate it, and make it wearisome. Doctor Johnson said that making dictionaries is dull work.

I say these things to indicate that I am no Ockhamist and that, within reason, I am content to follow most verbal signs wherever they may lead and however vague or blurred they may appear. The term fascism, however, provides no direction. I have attempted to say of it what Arthur O. Lovejoy many years ago said of romanticism: “The word romantic has come to mean so many things that, by itself, it means nothing. It has ceased to perform the function of a verbal sign.”¹ He preferred to speak of romanticisms, plural forms of what generally was interpreted as a single phenomenon. His advice was the traditional counsel of our profession: first understand each particular case, and the question of general concepts will take care of itself. Historians of fascism have received the same advice from their own masters. The way to understand Italian fascism, Renzo De Felice has taught, is to write its history.² Such projects in empirical research are the path along which De Felice and Karl Dietrich Bracher have led. One can commonly anticipate the results. The study of a particular movement in a particular place at a particular time generally leads to the conclusion that it was just that—

¹ Lovejoy, “On the Discrimination of Romanticisms,” *Publications of the Modern Language Association*, 39 (1924): 232.

² De Felice, *Interpretations of Fascism*, trans. Brenda Everett (Cambridge, Mass., 1977), 169–70.

particular, unique, and “not for export.” In history, facts and concepts are always uneasy bedfellows; in studies on fascism, no one has been able to get them together yet. What Lovejoy said of his various romanticisms, we can say of those various parties called fascism: “There may be some least common denominator of them all; but if so, it has never yet been clearly exhibited, and its presence is not to be assumed *a priori*.”³

Some critics will accuse me of making too much of the differences between these parties. Sometimes they make too much of the similarities. Stanley G. Payne remarks wisely that historical understanding requires that both receive equal consideration. Yet similarities can be deceiving and differences illusory. Professor Nolte observes that a group of men need not have a family likeness in order to be brothers. A prudent person faced with such strangers would, however, naturally demand some proof of their relationship. Indeed, he should demand proof even from men with the likeness of twins, for many who resemble each other are not brothers. This is more like it. The issue is not that so-called fascist movements look different but may be related; it is that they look alike but may not be related. That Nolte would fix upon this image of brothers different in appearance but alike under the skin is not unexpected. For him, what makes men fascist is something deep in the personality, emotions that—like the genes of blood brothers—are possessed in common and rooted in nature itself.⁴ Emotions, however, can be known to historians only by external expressions, and the words and deeds of alleged fascists, as I have remarked, make treacherous historical testimony. I have observed that Doriot and Mosley emphasized the national character of their movements; Nolte replies correctly that they acknowledged certain supranationalist characteristics as well. Numerous conclusions are possible: one is that they talked out of both sides of their mouth; another is that the differences are resolved by the paradoxical nature of fascism itself. Like our own century, explains Nolte (“at the risk of oversimplification”), which is neither clearly national nor international, fascism was nationalist and supranationalist at the same time. Further, we are told, it is also reactionary and revolutionary, bourgeois and populist, modern and antimodern. Some will agree that this, indeed, is a phenomenon of paradoxes, a unity of opposites with enough colors and contrasts to resolve any number of contradictions and inconsistencies. Others will conclude simply that the emperor has no clothes.

Words should not be permitted to become mysteries, symbols with a meaning obscure and indefinable. But some will always be more difficult than others. I confess that in my classroom I wield with abandon such terms as “constitutionalism” and “parliamentarianism,” and, therefore, I acknowl-

³ Lovejoy, “On the Discrimination of Romanticisms,” 236.

⁴ Nolte’s famous definition of fascism as primarily a form of anti-Marxism will not be discussed here. Interesting in this connection, however, are the remarks of Eugen Weber concerning what he has described as fascist movements in Rumania and even Hungary, areas where the workers were unorganized and the Communists weak and subdued. Here, where no significant enemy existed on the revolutionary Left, organizations such as the Legion of the Archangel Michael did not give anti-Marxism the high priority that it received among putative fascist groups in the West. See Weber, “The Men of the Archangel,” *Journal of Contemporary History*, 1 (1966): 104–05.

edge Professor Nolte's comments on these troublesome abstractions. Things that change over time and space are difficult to cage in precise terms. Yet, when pressed to define these constructs, I believe that I know the approximate ground upon which to proceed. Others may disagree with the result, but generally we will be in the same realm of discourse. The debate on fascism knows no common ground. If the literature on interpretations of fascism exposes the categories within which we perceive the problem, it reveals as well that most of them are irreducible. Functional definitions, historical definitions, phenomenological definitions, definitions launched from every political direction pass each other like ships in the night. The reason, put simply, is that mismatched phenomena continue to be called by one name. The word fascism appeared in history before the birth of many of the movements that claimed to be, or were accused of being, fascist. As the movements went their various ways, the word was stretched, abstracted, and inflated to perpetuate the illusion of their common identity. Nolte wishes to make a distinction between the commonplace and scholarly usage of the term. Both are a muddle. Unable to convert a sufficient following, most specialists continue with some private definition of their own. Most layman, on the other hand, continue without any definition at all: "We all know what it means."

Professor Nolte submits that I cannot do without the word fascism. I confess it. I have no other label for those "throngs of nationalist radicals" that he wants to pour into new bottles, nor do I have special information upon which to construct new theories about their substance. Anyway, the true complication is not the word but what has been put into it. As Professor Payne comments, fascism has been reified, over-abstracted, and pumped full of air. To mix metaphors, I would say that it fattened on our will to believe in a "central significance" (Nolte) uniting all supposed fascist forms. I have explained already that, as empirical studies continue to demonstrate the variety of these forms, prefixes continue to be employed to preserve the impression of their unity: the more studies, the more prefixes—"clericofascism," "anarchofascism," and so forth. The proliferation of prefixes does not disturb me, but the omnipotence of the noun does. Students of history will not think clearly on this subject as long as fascism is represented to them as an entity fundamentally uniform and absolute, something with hands and feet, a real presence incorporated in nature. Again, the problem is not the word but the power we give it. My message is that this power has increased, is increasing, and should be diminished.

WHAT IS TO BE DONE? Those who like to know what they are talking about cannot at present say "fascism" with a clear conscience, nor can they continue indefinitely the pedantry of placing the term in quotation marks or qualifying it with adjectives of their own: "so-called" fascism, "reputed" fascism, and the like. Cautious but still mildly confident, Professor Payne believes that hope remains for a "fascist minimum." He is not one to under-

estimate the task. Indeed, Solomon himself would be hard tested to devise a "multiform hypothetical category" for the fascism that Payne describes: a diversified phenomenon without "common ideology, common structure, common causes, or even common motivations." What Payne takes away from this phenomenon with one hand, however, he sometimes adds with the other. For example, his repeated description of these movements as "revolutionary" is likely to stir opposition on the Left. The debate over whether fascism was revolutionary or reactionary began on the day Mussolini became the first fascist; it is unlikely to end until historians agree on a definition for these two words as well.

Being no nominalist historian, however, I generally value the search for analytical categories. But I value little the kind of ecumenism involved in the search for a fascist minimum. "Minimums" make sense only where "essential" features can be distinguished from "externals" and where similarities outweigh or at least are roughly equal to differences. Otherwise, one is merely calculating the lowest common denominator of essentially discrepant phenomena.⁵ In studies of fascism the question remains open whether the parties involved were more alike or more dissimilar. I have noted that Professor Payne believes that the differences are as important to historical understanding as the similarities. I believe that they are more important. The hunt for a fascist minimum began among so-called fascists themselves at Montreux in 1934, when the movements came away universally disappointed with the result. Really, is there anything in the recent literature to justify the faith that we can now do better ourselves?

Some words, after all, can be returned to sanity. The term "totalitarianism," metamorphosed during the 1950s into superhistorical dimensions, has now largely been emptied of philosophic litter and is generally used with more economy and restraint. When word battles continue too long, however, some earnest scholars—perhaps like those keen but uncomfortable students described by Professor Payne—are likely to try to preserve their own sanity by disowning the whole enterprise. In Alfred de Musset's *Lettres de Dupuis et Cotonet*, the good citizen Dupuis, after twelve years of "suffering," despaired of the wrangles of Paris intellectuals over the meaning of romanticism and settled for the opinion of the rustics in the provinces. "In general," he explained, "we give the word romanticism a definition easy to remember: we say it is synonymous with absurd, and we worry no more about it."⁶

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⁵ Musset, *Oeuvres complètes en prose* (Paris, 1960), 822.

Review Article

The Bureaucratic Phenomena of Imperial Russia, 1700–1905

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THE APPEARANCE OF THE MONOGRAPH *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX veke* [The Governmental Apparatus of Autocratic Russia in the Nineteenth Century] by P. A. Zaionchkovskii, the distinguished Soviet chronicler of Russian political history of the last century, offers occasion to take stock of recent scholarship dealing with the government of the empire.¹ Indeed, since the 1950s in the West and more recently (and increasingly so) in the Soviet Union, substantial work has been done to obtain a comprehensive picture of the workings of imperial institutions and their personnel. Unquestionably, an exhaustive account of that literature cannot be given in an article, particularly since an excellent and quite full survey of the major published studies is available in an earlier review article by Daniel T. Orlovsky.² Furthermore, many recent contributions, based on extensive archival research, are still unpublished in doctoral dissertations and conference papers. My own modest aim here is to identify and assess the more significant conclusions reached by these studies and raise some questions for future research.

As is the case of most governments, but perhaps to a greater degree in imperial Russia, the working of the state apparatus and its personnel is intimately tied to the country's social and cultural life. Consequently, the dimension of time—the evolution of society and culture—should always be kept in mind; and, if it has done nothing else, the research of recent decades has graphically revealed the changing governmental structures behind a facade of seemingly immutable forms.³ At least two major periods, dividing from each other somewhere between 1815 and 1848, may now be distinguished. In the preparation of this essay I have used, along with published books and articles, a number of typescripts that were kindly shown to me. Naturally, the coverage is far from complete, since I am not in direct contact with all of the researchers active in this particular area of Russian history. I wish to thank the authors or editors of the unpublished material for the generosity and collegial spirit with which they have shared their work.

¹ Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.* (Moscow: "Mysl'," 1978; pp. 288).

² Orlovsky "Recent Studies on the Russian Bureaucracy," *Russian Review*, 35 (October 1976): 448–67.

³ For a formal overview of the major institutions of the empire, see Erik Amburger, *Geschichte der Behördenorganisation Russlands von Peter dem Grossen bis 1917*, Studien zur Geschichte Osteuropas, no. 10

in the evolution of the imperial regime. These two major epochs may, of course, be further subdivided; the reigns of Catherine II in the eighteenth century and of Nicholas I in the nineteenth were the seminal periods for the gestation of subsequent, far-reaching transformations. The dynamic picture thus obtained is the most significant achievement of recent scholarship, but there is still much dispute as to the basic forces and tensions that compose this dynamics.

A MAJOR REASON FOR THIS LACK OF CONSENSUS is the close connection that Peter the Great's Table of Ranks of 1722 established between the governmental apparatus and the country's social structure. As a result of that connection, much of the research and literature on government personnel gets entangled with the complex questions of the empire's social stratification and economic structure. Russian historians in the nineteenth and early twentieth centuries (mostly of "liberal" political persuasion), parroting West European bourgeois prejudices, saw in the imperial state apparatus a mechanism for social, economic, and cultural oppression by the nobility. Naturally, Marxism—that quintessential bourgeois ideology—followed suit, further emphasizing the evil, conspiratorial character of the apparatus. Imperial policies were thus easily explained away as measures taken in the interest of the serf- and land-owning nobility. Difficulties did arise, of course, in trying to account for the state's repeated espousal of and resort to policies inimical to the selfish class interests of the nobility, the most dramatic instance of which was the emancipation of the serfs in the reign of Alexander II. In this perspective, a major issue was the effectiveness of the Table of Ranks in tying membership in the imperial administration to noble status and privilege. Scholars have found themselves confronted with the problem of whether rank actually depended on service (as the legislation would have it) or on noble status (whatever that meant in the Russian context—quite a vexing problem in itself). A related issue was the economic status of government officials: Did high rank entail wealth or did great wealth facilitate attainment of high rank? These problems have not been easy to resolve since the available documentation has been spotty and unsatisfactory. Literary evidence and *obiter dicta* provided the grounds for generalizations that clearly had political implications. But some real progress has recently been made in this area of concern.

Following the pioneer effort of Walter M. Pintner, several scholars—the late Sergei M. Troitskii, M. D. Rabinovich, and Brenda Meehan-Waters for the eighteenth century and Richard S. Wortman, W. Bruce Lincoln, and now P. A. Zaionchkovskii for the nineteenth—have studied the socio-economic

(Leiden: E. J. Brill, 1966; pp. xxxii, 622); and N. P. Eroshkin, *Istoriia gosudarstvennykh uchrezhdenii dorevoliutsionnoi Rossii* (2d ed., Moscow: "Vyssh. shkola," 1968; pp. 368). For a descriptive analysis of the legislation pertaining to government officials in the first half of the nineteenth century, see Hans-Joachim Torke, "Das russische Beamtentum in der ersten Hälfte des 19. Jahrhunderts," in *Forschungen zur Osteuropäischen Geschichte*, 13 (Wiesbaden: Otto Harrassowitz, for Osteuropa-Institut an der Freien Universität Berlin, 1967): 7-345.

profiles of select groups of Russian government and army officials.⁴ Service records found for some institutions have enabled these scholars to reconstruct career patterns and establish the social background and economic circumstances of some officials. We should note, first, that the bulk of this unfortunately spotty evidence pertains to officials in the higher ranks, whose lives and fortunes were naturally better documented than those of their subordinates or "inferiors." These sources, furthermore, are insufficient to trace the antecedents of the officials studied, so that with the exception of very prominent families we rarely have meaningful information for more than one generation. Thus, it is almost impossible to establish whether a given official or group of officials descended from the old Boiars or merely from families recently ennobled through service.⁵ The same difficulty arises in connection with property: had it been in the family for several generations or was it newly acquired as a result of service opportunities? The sources rarely tell us. In spite of these limitations, certain broad conclusions do emerge that provide specific data and some statistical information (for those who put greater faith in quantitative over qualitative evidence) to support previous generalizations and to qualify *idées reçues*.

From these scholars' findings the officials in the Russian imperial government clearly constituted a distinct social group that expanded in arithmetic progression throughout the eighteenth century and grew in geometric progression in the nineteenth.⁶ The groups' membership was relatively open to newcomers and its status was not exclusively dependent upon birth, family, or wealth. These studies have also conclusively demonstrated that the bulk of the officials owned neither serfs nor land (or owned them in negligible amounts), although the highest dignitaries often were, of course, quite wealthy. To infer therefrom that state service (including favoritism and patronage) provided access to wealth rather than that wealth was a precondition for successful service would be reasonable. On the basis of admittedly fragmentary evidence Zaionchkovskii concluded, incidentally, that Russian officialdom at the end of

⁴ Troitskii, *Russkii absolutizm i dvorianstvo v XVIII v.: Formirovanie burokratii* (Moscow: Izdatel'stvo "Nauka," 1974; pp. 394); Rabinovich, "Sotsial'noe proiskhozhdenie i imushchestvennoe polozhenie ofitserov reguliarnoi russkoi armii v kontse Severnoi voiny," in N. I. Pavlenko, L. A. Nikiforov, and M. Ia. Volkov, eds., *Rossia v period reform Petra I* (Moscow: Izdatel'stvo "Nauka," 1973; pp. 384), 133-71; Meehan-Waters, *Autocracy and Aristocracy: The Russian Elite, 1689-1761* (forthcoming); Wortman, *The Development of a Russian Legal Consciousness* (Chicago: University of Chicago Press, 1976; pp. xi, 345); Lincoln, *The Tsar's Most Loyal Servitors: Russia's Enlightened Bureaucracy (1825-1881)* (forthcoming); and Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.* For the first important study by Pintner, see his "The Social Characteristics of the Early Nineteenth-Century Russian Bureaucracy," *Slavic Review*, 29 (September 1970): 429-43; and, for a convenient summary of these and subsequent findings, see Walter M. Pintner and Don Karl Rowney, eds., *Russian Officialdom from the 17th to the 20th Century: The Bureaucratization of Russian Society* (Chapel Hill: University of North Carolina Press, forthcoming), chaps. 8, 9.

⁵ Genealogies are particularly difficult to trace in the case of the Russian nobility. For a good introduction to that thorny problem by its most distinguished expert in recent times, see S. B. Veselovskii, *Issledovanie po istorii klassa sluzhilykh zemleladel'tsev* (Moscow: Izdatel'stvo "Nauka," 1969; pp. 582), esp. 7-38.

⁶ In 1796 there were fifteen to sixteen thousand officials for a population of approximately thirty-six million; in 1851 there were over seventy-four thousand officials for a population of about sixty-nine million; and in 1903 there were about three hundred and eighty-five thousand officials for a population (according to the census of 1897) of one hundred and twenty-nine million. Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.*, 221.

the nineteenth century did not get much involved in capitalist forms of wealth (shares, stocks, factories, and the like) and that apparently the regulations against conflicts of interest in this form were rather effective.⁷

Just as a bottle may be viewed as either half empty or half full, historians can say, on the one hand, that the Petrine legislation was ineffective because so many officials (especially in the upper ranks) were nobles or, on the other, that it was effective because so many other officials (especially in the middle and lower echelons) were of nonnoble backgrounds. Naturally, over several generations members of Russia's officialdom had a better chance for promotion to the upper ranks than did outsiders in entering those ranks and, thus, came to constitute the upper stratum of service, especially since rank (according to the Table of 1722) did entail noble status until the end of the imperial regime in spite of increasingly restrictive patterns of promotion. Nor should it be forgotten, as we are reminded by Helju A. Bennett,⁸ that decorations and knightly orders awarded for length of service also conferred noble status; longevity in service in itself could, therefore, be the first step of a successful career pattern for the children and grandchildren of an official. In the eighteenth century the high proportion of nobles in service, despite the "democratic" provisions of the Petrine Table of Ranks, should not be surprising, especially in view of the role of the military in the administration.⁹ The Russian state had no alternative sources for recruiting its officials; unlike the *anciens régimes* of Prussia and France, Russia did not have a rural or an urban "bourgeoisie," petty local officials (patrimonial or provincial), learned professions (clerical, legal, or medical), and so forth. This restricted pool for recruitment helps explain the employment of the children of soldiers and clergy along with ethnic outsiders (the Ukrainians, for instance), the common practice of the Russian government from the late eighteenth century onward. Naturally, after 1861 recruitment into the lower ranks of government service was expanded still further to include the new social groups that emerged as a consequence of the empire's rapid modernization.

Although the tsar's service in Muscovy had provided land and serfs, after Peter the Great these perquisites were no longer automatic. In the course of the eighteenth century this pattern became even less normal, and it virtually disappeared during the nineteenth. To accept the notion that the imperial service class was representative of the serf- and land-owning nobility is, therefore, difficult. And did officials necessarily have to represent the economic interests of others? Did they have to serve as instruments of another class? This conspiratorial view of government, held by the French liberals and revolutionaries of the late eighteenth and early nineteenth centuries, was

⁷ For the situation in the eighteenth century, see Troitskii, *Russkii absoliutizm i dvorianstvo v XVIII v.*; and, for the end of the nineteenth century, see the data collected in Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.*, chap. 4.

⁸ Bennett, "Chiny, Ordena, Autocracy: Institutions Affecting Officialdom in Russia," in Pintner and Rowney, *Russian Officialdom from the 17th to the 20th Century*, chap. 7.

⁹ Robert D. Givens, "Eighteenth-Century Nobiliary Career Patterns and Provincial Government," in Pintner and Rowney, *Russian Officialdom from the 17th to the 20th Century*, chap. 5.

taken over by that most representative of bourgeois ideologists Karl Marx, and, couched in general terms, became unquestioned truth for many. But, as Roland Mousnier has effectively argued, economic interests were not necessarily the main motive for group behavior; prior to the development of "modern" economic structures, status and the intangibles of glory and prestige could be just as important.¹⁰ Status conferred by state service was crucial in the Russian case, for the government was quite parsimonious about material compensation while very generous in providing outward marks of authority that, incidentally, could in turn provide opportunities for graft—a significant item in view of the low salaries.¹¹ This consideration justifies the inclusion in officialdom of clerks and other officials below those who comprised the hierarchy in the Table of Ranks; for these more lowly servants of the state shared with their superiors an authority and the extralegal sources of revenue denied anyone outside of officialdom, and their children frequently enjoyed preferential treatment if they followed in their fathers' paths.¹² Their inclusion received legal recognition and appropriate regulation through the creation of chancery clerkships in the reign of Nicholas I.¹³

In view of this situation and the rapid expansion of the number of officials, the highest-ranking dignitaries were in a particularly favorable position to perpetuate their pre-eminence. The personal nature of the autocrat's power and the limited circle from which Peter the Great and his successors could draw their servants tended to restrict the candidates for high office to members of those older families whose role in the government had become traditional.¹⁴ Quite naturally, these officials endeavored to preserve their position by inbreeding and socially discriminating against newcomers.¹⁵ Thus, a relatively small number of families managed to perpetuate themselves in positions

¹⁰ Mousnier, *Les Institutions de la France sous la monarchie absolue, 1598-1789*, vol. 1: *Société et état* (Paris: Presses Universitaires de France, 1974; pp. 586), esp. bk. 1, and *Les Hiérarchies sociales de 1450 à nos jours* (Paris: Presses Universitaires de France, 1969; pp. 196). Also see the comparative material in Roland Mousnier, ed., *Problèmes de stratification sociale: Actes du colloque international, 1966*, Publications de la Faculté des Lettres et Sciences humaines de Paris-Sorbonne, série "Recherches," vol. 43, Travaux du Centre de Recherches sur la Civilisation de l'Europe moderne, no. 5 (Paris: Presses Universitaires de France, 1968; pp. 284).

¹¹ For several illustrations of budgets of individuals and of salary scales, see Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.*, 81-90.

¹² Troitskii, *Russkii absolutizm i deianstvo v XVIII v.* For the problem of bureaucratic morality in particular, see Torke, "Das russische Beamtentum in der ersten Hälfte des 19. Jahrhunderts," 224.

¹³ Harold A. McFarlin, "The Extension of the Imperial Russian Service to the Lowest Office Workers: The Creation of the Chancery Clerkship, 1827-1833," *Russian History*, 1, pt. 1 (1974): 1-17.

¹⁴ Recent scholarship, in particular the studies of Robert O. Crummey, has conclusively demonstrated the dramatic expansion of the Muscovite governmental apparatus in the second half of the seventeenth century. See, for example, Crummey, "The Origins of the Noble Official: The Boiar Elite, 1613-1689," in Pintner and Rowney, *Russian Officialdom from the 17th to the 20th Century*, chap. 3, and "The Reconstitution of the Boiar Aristocracy, 1613-1645," in *Forschungen zur osteuropäischen Geschichte*, 18 (Wiesbaden: Otto Harrassowitz, for Osteuropa-Institut an der Freien Universität Berlin, 1973): 187-220; the papers given at the Conference on Muscovite and Medieval Russia held at Berlin in June 1978, forthcoming in *Forschungen zur osteuropäischen Geschichte*; and Borivoj Plavsic, "Seventeenth-Century Russian Chanceries and Their Staff," in Pintner and Rowney, *Russian Officialdom from the 17th to the 20th Century*, chap. 2. For statistical data that is now available, see Ia. E. Vodarskii, *Naselenie Rossii v kontse XVII-nachale XVIII veka (Chislennost', soslovno-klassovyi sostav, razmeshchenie)* (Moscow: Izdatel'stvo "Nauka," 1977; pp. 262).

¹⁵ Only a small number of the foreigners taken into imperial service in the reign of Peter the Great were assimilated into the Russian "aristocratic elite" in the first half of the eighteenth century.

of decisive importance at court and in the top echelons of government. These dignitaries, in turn, had their own clienteles and cliques, whose members they tried to place in strategic service positions. The dignitaries' own positions were, however, as often as not determined by the sovereign's favor. The resultant personal insecurity, instability, and rivalry within this group largely explains why it did not constitute an oligarchy *sensu stricto*—the attempt of 1730 in that direction ended in complete failure.¹⁶ It is the particular merit of Brenda Meehan-Waters to have shown the survival of this pattern from Muscovite to post-Petrine times and of David L. Ransel to have illustrated its operation during the reign of Catherine II.¹⁷ Jean-Pierre LeDonne has rounded out the picture by demonstrating that service with specific cliques of dignitaries substantially influenced the recruitment and appointment of senators and governors.¹⁸ Similar studies are needed to verify the impression that the pattern continued in the nineteenth century, at least at court and among the innermost circles of imperial advisors. Although not necessarily entrusted with administrative positions, those most influential around the ruler not only formed cliques but were themselves members of, or associated through marriage with, clans that had been prominent in this manner.¹⁹ Only occasionally did a complete outsider—like General A. A. Arakcheev or General A. Kh. Benkendorf, for example—reach such an official position of influence; and the general scorn in which such an outsider was held often arose less from his policies than from his status as an upstart or outsider. One might perhaps push the point further and ask whether the well-attested, interdepartmental rivalries (for example, interior versus finance) at the end of the nineteenth century were not an updated form of the clique politics that had dominated the eighteenth century. The events of the last decade of the reign of Nicholas II would seem to provide indirect evidence that such a situation still obtained and that it contributed significantly to the violent end of the imperial regime.

THE QUESTION OF THE DEMISE OF THE IMPERIAL REGIME leads to the often-mentioned, but not well-studied, central problem of Russian political history and thought, that of the nature of the autocrat's power and role. In recent years some work has been done on this facet of the imperial regime, and older

¹⁶ For the most recent account of the crisis of 1730, see James Cracraft, "The Succession Crisis of 1730: A View from the Inside," *Canadian American Slavic Studies*, 12 (Spring 1978): 60–85. Also see Meehan-Waters, *Autocracy and Aristocracy: The Russian Elite, 1689–1761*, chap. 6.

¹⁷ Meehan-Waters, "The Muscovite Noble Origins of the Russians in the Generalitet of 1730," *Cahiers du monde russe et soviétique*, 12 (January–March, 1971): 28–75, and *Autocracy and Aristocracy: The Russian Elite, 1689–1761*, chap. 6; and Ransel, *The Politics of Catherine's Russia: The Panin Party* (New Haven: Yale University Press, 1975; pp. x, 327).

¹⁸ LeDonne, "Appointments to the Russian Senate, 1762–1795," *Cahiers du monde russe et soviétique*, 16 (January–March 1975): 27–56, and "The Evolution of the Governor's Office, 1727–1764," *Canadian American Slavic Studies*, 12 (Spring 1978): 86–115.

¹⁹ We need a study along these lines of the so-called Unofficial Committee of Alexander I and of the "Senatorial Party" in the same reign as well as of the prominent members of the court and the Ministry of Domains in the reigns of Alexander II and Alexander III. Equally important is a prosopographical investigation of the emperor's *fligel' ad'utanty*.

studies exist for the earlier period.²⁰ The late Michael Cherniavsky made several provocative observations about the image of the ruler and its relationship to the people.²¹ Bruce Lincoln, P. A. Zaionchkovskii, and Theodore K. Taranovski have tried to describe and analyze the autocrat's authority and role in the cases of Nicholas I and Alexander III.²² So far, however, these efforts have largely focused on specific personalities of rulers and favorites and have tended, as a consequence, to underscore individual subjective traits. Yet, as Taranovski has made clear in his study, the power of the autocrat as an institution remained intact throughout the entire imperial regime. The institutional authority of autocracy proved to be decisive at strategic moments in the regime's history—for example, during the Decembrist revolt, the emancipation of the serfs, and “counterreforms” of the 1880s, and the aftermath of 1905.

One thing is, however, clear: inasmuch as the officials increasingly depended upon service for their livelihood, responsibility, and status, they were loathe to abandon their special personal relationship to the sovereign. Their reluctance is particularly understandable since no regularized system of law and judicial hierarchy protected them in the performance of their duties or safeguarded them from the consequences of even routine actions.²³ Furthermore, as the officials did not represent—or identify with—any social or economic class, they had no power base outside that of the autocrat. They developed their professional ethos and personal system of values entirely in terms of their loyalty to the person of the ruler and their function as his representatives and executors.²⁴ Until the late eighteenth century this attitude was further reinforced by the great insecurity of person and property to which the dignitaries and their clienteles were exposed (without any regard for their “noble” status). As Meehan-Waters has shown so well, confiscation of property, exile, and imprisonment were the ever-present threats (and all too often the lots as well) of the seemingly high and mighty in the first half of the

²⁰ M. A. D'iakonov, *I'last' moskovskikh gosudarei (Ocherki iz istorii politicheskikh idei drevnei Rusi do kontsa XVI veka)* (St. Petersburg: Tip. I. N. Skorokhodova, 1899; reprint ed., The Hague: Mouton, 1969; pp. 224); Vladimir Val'denberg, *Drevnerusskie ucheniia o predelakh tsarskoi vlasti* (Petrograd: [A. Benke] 1916; reprint ed., The Hague: Europe Printing, 1966; pp. 463); and Helmut Neubauer, *Car und Selbstherrscher: Beiträge zur Geschichte der Autokratie in Russland* (Wiesbaden: Otto Harrassowitz, 1964; pp. 236).

²¹ Cherniavsky, *Tsar and People: Studies in Russian Myths* (New Haven: Yale University Press, 1961; pp. xix, 258).

²² Lincoln, *Nicholas I, Emperor and Autocrat of All the Russias* (Bloomington: Indiana University Press, 1978; pp. 424); Zaionchkovskii, *Rossiiskoe samoderzhavie v kontse XIX stoletia (Politicheskaiia reaktsiia 80kh-nachala 90kh godov)* (Moscow: “Mysl”, 1970; pp. 442), esp. chap. 1; and Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III, 1881–1894” (Ph.D. dissertation, Harvard University, 1976).

²³ Of particular interest in this connection are the diaries of the censor, A. V. Nikitenko, and those of the ministers P. A. Valuev (1861–76) and D. A. Miliutin (1873–82). See Nikitenko, *Dnevnik*, ed. I. Ia. Aizenshtok, 3 vols. (Leningrad: Gosudarstvennoe izdatel'stvo khudozhestvennoi literatury, 1955–56; pp. xlii, 541, 650, 580); Valuev, *Dnevnik P. A. Valueva, ministra vnutrennikh del*, ed. P. A. Zaionchkovskii, 2 vols. (Moscow: Izdatel'stvo “Nauka,” 1961; pp. 420, 586); and Miliutin, *Dnevnik D. A. Miliutina*, ed. P. A. Zaionchkovskii, 4 vols. (Moscow: [Tipografiia Biblioteki imeni V. I. Lenina] 1947–50; pp. 253, 290, 323, 201).

²⁴ Torke, “Das russische Beamtentum in der ersten Hälfte des 19. Jahrhunderts”; and Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III,” esp. chaps. 1, 2.

eighteenth century.²⁵ The sense of insecurity waned in the reign of Catherine II, but it revived under Paul I. With Alexander I and his successors, the life and property of the officials became more secure but their position and status did not; the old syndrome of insecurity was not easily overcome. As Taranovski has shown, concern for one's position in the hierarchy remained a powerful, even the determining, factor in the behavior and decisions of high officials to the very end of the regime.²⁶ Manipulation of this situation enabled Alexander II to get the emancipation enacted and Alexander III to implement the policy of counterreforms.²⁷ The absence of a genuine code and the inadequacies of the judiciary system—even after its thoroughgoing reform in 1864—only served to perpetuate the conditions that permitted the autocrat to preserve the full range of his personal power for intervention and decision, precluding the development of a true *Rechtsstaat* guaranteeing the security of officials as well as of the subjects.²⁸

AMONG THE MOST IMPORTANT FINDINGS OF RECENT SCHOLARSHIP is the changing nature of Russian officialdom, more particularly the seminal role of the transformations in the first half of the nineteenth century. The impression gained, which needs underpinning through further detailed study, is that the groundwork for change was laid in the reign of Alexander I, although the much better-documented reign of Nicholas I logically extended and brought to completion the process begun at the start of the century. First, officialdom separated from the serf- and land-owning class, even from educated “society” *tout court*. Indeed, an educated elite whose members led private lives independent of the government arose in Russia for the first time during the reign of Alexander I. The most visible signs of that process are the emergence of professional writers (Nikolai Karamzin and Alexander Pushkin, for example), of literary societies (*Arzamas*, “Friendly Literary Society,” and the like), and of the growing number of public lectures and other academic interests among the upper classes of the two capitals.²⁹ These manifestations were accom-

²⁵ Meehan-Waters, *Autocracy and Aristocracy: The Russian Elite, 1689–1761*, chap. 4, and her forthcoming article on the political trials and confiscations in the eighteenth century, “Elite Politics and Autocratic Power,” in Anthony G. Cross, ed., *Great Britain and Russia in the Eighteenth Century—Comparisons and Contrasts* (Newtonville, Mass.: Oriental Research Partners, forthcoming).

²⁶ Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III,” esp. chap. 5.

²⁷ Daniel Field, *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855–1861*, Russian Research Center Studies, no. 75 (Cambridge: Harvard University Press, 1976; pp. viii, 472); Zaionchkovskii, *Rossiiskoe samoderzhavie v kontse XIX stoletia*; and Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III,” chap. 5.

²⁸ On the problem of codification, see Wortman, *The Development of a Russian Legal Consciousness*; and Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III,” esp. chaps. 2, 3. For an overview of the nature of Russian legal materials, see P. A. Zaionchkovskii, ed., *Spravochniki po istorii dorevoliutsionnoi Rossii: Bibliograficheskii ukazatel'* (2d ed., Moscow: “Kniga,” 1978; pp. 639), 23–36.

²⁹ Iu. M. Lotman, “Dekabrist v povsednevnoi zhizni (Bytovoe povedenie kak istoriko-psikhologicheskaiia kategoriia),” in V. G. Bazanov and V. E. Vatsuro, eds., *Literaturnoe nasledie dekabristov* (Leningrad: Izdatel'stvo “Nauka,” Leningradskoe otdelenie, 1975; pp. 400), 25–74; and V. G. Bazanov, *Ucheniia respublika* (Leningrad: Izdatel'stvo “Nauka,” Leningradskoe otdelenie, 1964; pp. 461).

panied by a growth in the number of officials and their increasing professionalization. One of the landmarks of this process was the set of acts that made educational achievement (as tested by examinations) a requirement for promotion.³⁰ That this legislation was strenuously resisted and frequently breached is certainly true, but in the long run it proved effective in changing the character of officialdom.³¹ Education increasingly became a requirement for any member of the elite, most particularly for those who served the state. To make fulfilling these educational requirements possible, new universities and elite schools (such as the *lycées* of Bezborodko and Tsarskoe Selo) were established and new disciplines (such as public law, statistics, and political economy) were introduced into the curriculum at the same time that academic contacts with the West were intensified and expanded.³²

As Pintner, Lincoln, and Wortman have well documented and the material in Zaionchkovskii's monograph confirms, the trend was reinforced in the reign of Nicholas I—a reign that, despite its “conservative” or even “reactionary” character and stultifying spiritual atmosphere, was seminal in the development of professional and technical education in Russia.³³ During this period the first generation of “liberal” professional bureaucrats was trained and formed. This group was entirely dependent on the state for its livelihood and careers and exclusively devoted to the good and interests of the country, insofar as those bureaucrats conceived that good and these interests.³⁴ Noteworthy also is the training of a group of legal experts (a by-product of Michael Speranskii's efforts at codification), who subsequently produced the great judiciary reform of 1864 and introduced some of the notions and ideals of a *Rechtsstaat* into Russian public life after 1861.³⁵ That these legal experts were so few and that they did not take control of the bureaucratic apparatus (largely because codification was not brought to full conclusion) served to preserve the traditional role of the autocrat and to maintain the older ethos and methods intact.³⁶ As a result, additional strain and conflict developed within the ranks of officialdom, and administrative arbitrariness continued to reign unfettered.

This process also resulted in a sharper cleavage between the staffs in the

³⁰ For the best account, see Torke, “Das russische Beamtentum in der ersten Hälfte des 19. Jahrhunderts,” chap. 3.

³¹ Harold A. McFarlin has an interesting reading of Gogol's “Overcoat” in light of the legislation on educational requirements for promotion; see his forthcoming article in *Canadian American Slavic Studies*.

³² James T. Flynn, “The Universities, the Gentry, and the Russian Imperial Services, 1815–1825,” *Canadian Slavic Studies*, 2 (Winter 1968): 486–503, and “The Universities in the Russia of Alexander I: Patterns of Reform and Reaction” (Ph.D. dissertation, Clark University, 1964). His monograph on the subject, including the reign of Nicholas I, will, I hope, be published soon.

³³ In addition to the works of these scholars cited above, see Nicholas Riasanovsky, *A Parting of the Ways: Government and the Educated Public in Russia, 1801–1855* (New York: Oxford University Press, 1976; pp. viii, 323).

³⁴ W. Bruce Lincoln, “The Genesis of an ‘Enlightened’ Bureaucracy in Russia, 1825–1856,” *Jahrbücher für Geschichte Osteuropas*, new ser., 20 (September 1972): 321–30, and *The Tsar's Most Loyal Servitors: Russia's Enlightened Bureaucracy (1825–1881)*.

³⁵ Wortman's *The Development of a Russian Legal Consciousness* has superseded all previous studies on that subject.

³⁶ For an illuminating analysis of the *Polizeistaat* and *Rechtsstaat* orientations within imperial officialdom, see Taranovski, “The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III,” esp. chap. 3.

central governmental institutions and those in the provincial administrative offices.³⁷ A functional specialization developed, first among central and local officials and then within departments like finance, interior, and transportation. The "generalist" of the eighteenth century who moved from military to civil duties and from central to local offices and back again became increasingly rare. The results were not necessarily beneficial; in the absence of serious prospects for promotion to the central institutions, the local officials became an inferior category of second raters, stuck in routine and afraid of change. Their shortcomings delayed the "systematization of government," in George L. Yaney's somewhat cumbersome but correct phrase,³⁸ and the inability of the central government to reach all of its subjects effectively proved to be a serious handicap to the implementation of reforms and to the economic modernization of the country. Richard G. Robbins, Jr., Taranovski, and Zaionchkovskii have shown that the local administrations, especially the governors and their staffs, were the main support of the truly reactionary efforts to undo the reforms of Alexander II and to turn the clock back in the 1880s and 1890s.³⁹ They exacerbated the conflict between "government" and "society" and dangerously eroded the willingness of the dynamic elements of society to support the regime.

On the basis of studies by Lincoln and Taranovski, which find graphic illustration in the detailed analyses of the emancipation by P. A. Zaionchkovskii, Terence Emmons, and Daniel Field, Russian officialdom from the middle of the nineteenth century divided into "liberal" and reform-minded administrators and conservative, though not necessarily reactionary, officials.⁴⁰ Whether the split was fundamentally ideological in nature (as Taranovski has suggested) may be questioned—at least until further evidence is amassed. But a split obviously did occur, and it did interfere with the proper implementation of the great reforms and the smooth functioning of administration and did enable Alexander III to introduce his nefarious counter-

³⁷ Pintner, "The Social Characteristics of the Early Nineteenth-Century Russian Bureaucracy"; Zaionchkovskii, *Pravitel'stvennyi apparat samoderzhavnoi Rossii v XIX v.*; Taranovski, "The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III," esp. chap. 5; and Henry Hirschbiel, "A Social Profile of Local Administration in Nikolaevan Russia: Kiselev's District Captains," unpublished paper. (Hirschbiel's dissertation on the same topic was unavailable to me; Hirschbiel, "The District Captains of the Ministry of State Properties in the Reign of Nicholas I: A Case Study of Russian Provincial Officialdom, 1838–1856" [Ph.D. dissertation, New York University, 1978].) For general background, see S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870* (Princeton: Princeton University Press, 1972; pp. xiii, 386); and, for a particular instance, see Jean-Pierre LeDonne, "La réforme de 1883 au Caucase: Un exemple d'administration régionale russe," *Cahiers du monde russe et soviétique*, 8 (January–March 1967): 21–35.

³⁸ Yaney, *The Systematization of Russian Government: Social Evolution in the Domestic Administration of Imperial Russia, 1711–1905* (Urbana: University of Illinois Press, 1973; pp. xvi, 430).

³⁹ Zaionchkovskii, *Rossiiskoe samoderzhavie v kontse XIX stoletia*; Robbins, *Famine in Russia, 1891–92: The Imperial Government Responds to a Crisis*, Studies of the Russian Institute, Columbia University (New York: Columbia University Press, 1975; pp. xiv, 262); and Taranovski, "The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III." Also see Robbins's forthcoming biography of Count Plehve.

⁴⁰ Zaionchkovskii, *Provedenie v zhizn' krest'ianskoi reformy 1861 g.* (Moscow: Izdatel'stvo sotsial'no-ekonomicheskoi literatury, 1958; pp. 468); Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge: Cambridge University Press, 1968; pp. ix, 483); and Field, *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855–1861*.

reforms.⁴¹ That split surely also contributed to the failure of a *Rechtsstaat* to emerge in Russia. It is indeed arguable that, in the absence of clearly defined administrative rules and fully codified law, a genuine bureaucratic system could not develop, despite the presence of many particular phenomena of bureaucracy.⁴² Almost in spite of themselves, both Wortman and Taranovski, who have shown the presence of a legal consciousness among some high officials and who have tried hard to make the judiciary reform of 1864 a major turning point in the history of the imperial system, are led to chronicle and document the ultimate failure of the struggle for a rule of law. Imperial Russia had become a *Reglementsstaat* but never became a *Rechtsstaat*; the emperor's *Machtspruch* remained an ever-present reality, even if it was not always invoked.

Crucial to the nature and role of government officials in determining the destinies of the country was their relationship to the people. How did this bureaucracy work? How did the various offices and institutions of the government, both local and central, operate? Only the very first steps have been taken so far to answer these questions—and only for the later period of the empire at that.⁴³ Several dissertations and monographs have been concerned with specific institutions (the ministries of education and interior or the office of the land captain, for example),⁴⁴ and some important administrative events have also received monographic treatment (like the governmental response to the famine of 1891).⁴⁵ Detailed descriptions of the elaboration of significant legislation have been undertaken and some results are available on the emancipation of the serfs, the establishment of the zemstvos, the reform of the judiciary, the implementation of the counterreforms, the revision of the election rules, and the institution of Peter Stolypin's agrarian reforms. But what is needed are concrete analyses of the relationship between officials and subjects, of the images and perceptions on the part of both administrators and administered, and of the mythology of power. Literature can, of course, be revealing in many respects; but the works of Michael Saltykov-Shchedrin, Vladimir Korolenko, and Alexander Kuprin as well as of Nikolai Gogol and Fedor Dostoevsky (to name but a few) are fraught with polemical and ideological distortions and frequently merely serve to reinforce stereotypes rather than to come to grips with reality.

⁴¹ Taranovski, "The Politics of Counter-Reform: Autocracy and Bureaucracy in the Reign of Alexander III," chap. 5.

⁴² I am, of course, aware of the danger of using Prussia as a yardstick or setting up an ideal type against which to measure historical reality. But for heuristic purposes it can be very suggestive.

⁴³ Jean-Pierre LeDonne's comprehensive study of eighteenth-century administration will, no doubt, fill the gap for the earlier period. In the meantime, we have to rely upon the formal descriptive handbooks. See L. A. Steshenko and K. A. Sofronenko, *Gosudarstvennyi stroi Rossii v pervoi chetverti XVIII veka* (Moscow: Izdatel'stvo Moskovskogo universiteta, 1973; pp. 120); and A. V. Chernov, *Gosudarstvennye uchrezhdenia Rossii v XVIII veke (Zakonodatel'nye materialy): Spravochnoe posobie* (Moscow: Moskovskii gosudarstvennyi istoriko-arkhivnyi institut, 1960; pp. 579).

⁴⁴ Allen Sinel, *The Classroom and the Chancellery: State Educational Reform in Russia under Count Dmitry Tolstoi*, Russian Research Center Studies, no. 72 (Cambridge: Harvard University Press, 1973; pp. xii, 335); and J. I. Mandel, "Paternalistic Authority in the Russian Countryside, 1856–1906" (Ph.D. dissertation, Columbia University, 1978).

⁴⁵ Robbins, *Famine in Russia, 1891–1892: The Imperial Government Responds to a Crisis*.

A significant facet of the interaction between government and population is the relationship between officials and society. Russian historians have assumed that, from the eighteenth century onward, and more particularly since the reign of Nicholas I, government and society starkly opposed one another. This opposition allegedly existed even when officialdom presumably consisted primarily of nobles; yet the nobles of that time perceived the official as an enemy. The conflict between the nobility and officialdom came into the open at the time of the elaboration of the great reforms under Alexander II. The officials—that is, the government—were seen as the enemies of society—that is, the serf- and land-owning nobility. And in the short run most reforms were certainly detrimental to the so-called ruling class of imperial Russia. Little wonder, therefore, that the bureaucracy—the state—came to be viewed as the primary enemy not only by the intelligentsia and revolutionaries but also by educated society, mainly the landowners and professional people. This vast subject still awaits its investigators, though such studies as those by Emmons on the emancipation and Robbins on the famine have done a great deal to bring the topic into focus and to illuminate some of its features. As recent dissertations written under the direction of Leopold H. Haimson have shown, Russian officials (however misguided their actual policies were) by and large were more concerned with peasants and workers than with the bourgeoisie, the landowners, and the nobles.⁴⁶ A policy pattern that came home to roost began under Stolypin, who endeavored to restructure public life by striking an alliance with the land-owning nobility (at least their economically viable sector).

This situation also lay at the root of the conflicts between local society and local officials and the ambivalences that arose in connection with devising local administrative structures. The unhappy history of the *zemstvos* and of the various efforts to delegate local administrative functions to truly representative individuals amply illustrate these difficulties. The main cause of these unfortunate developments was that, in contrast to the practices of England, Prussia, and post-Napoleonic France, the Russian imperial regime could not, and did not wish to, draw on the services of local notables. Hence, the officials (both central and local) remained distinct from the elite of society. The realities of government were not those of society, and the languages expressing the two realities were never the same.⁴⁷ The lack of an adequately codified system of law that would have provided one system of linguistic expression for

⁴⁶ Jeremiah Schneiderman, *Sergei Zubatov and Revolutionary Marxism: The Struggle for the Working Class in Tsarist Russia* (Ithaca: Cornell University Press, 1976; pp. 401); Roberta T. Manning, "The Russian Provincial Gentry in Revolution and Counter-Revolution, 1905-1907" (Ph.D. dissertation, Columbia University, 1975); and David Macey, "The Russian Bureaucracy and the 'Peasant Problem': The Pre-History of the Stolypin Reform, 1861-1907" (Ph.D. dissertation, Columbia University, 1976). And, of course, see Reginald E. Zelnik, *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855-1870* (Stanford: Stanford University Press, 1971; pp. 450). Zelnik's sequel is eagerly awaited.

⁴⁷ Iu. M. Lotman, "O Khlestakove," in B. Egorov et al., eds., *Tartu Riikliku Ülikooli Toimetised*, no. 369, *Trudy po russkoi i slavianskoi filologii*, no. 26 (Tartu: Tartuskii gosudarstvennyi universitet, 1975; pp. 208), 19-53.

common discourse is again a basic cause, as well as a telling illustration, of Russia's peculiar circumstance in this respect.⁴⁸

BECAUSE THE BUREAUCRACY, SUCH AS IT WAS, remained at the mercy of an autocrat, it could not consistently carry out long-range policies of transformation and reform. That bureaucracy was also separate and distinct from the society it administered. Russia developed almost none of the intervening links that can be identified in Western Europe, nor did Russia possess a common legal language that could serve as a medium of communication between society and the state. Russian officials constituted a class that was in conflict with both the source of its authority and the subjects of its concerns. These are the major conclusions of the historical literature here examined.

The career patterns, economic status, and social origins of the bureaucratic personnel have now been sufficiently elucidated, even adequately quantified; the autonomy of the officials as a separate group has been duly established. We must turn the page and ask new questions. The basic puzzle of how the imperial system functioned so long and so well in preserving the regime, even adapting to changed circumstances, is still a major challenge to historians of Russia. We realize now that the building stones of a bureaucratic edifice were present, but as of 1905 (or even 1917) imperial Russia still did not have a genuine bureaucracy. When the history of Russia is compared to that of Western and Central European nations, a persistent problem arises: why did the distinct elements of a bureaucracy fail to fuse into a structured system? Is not the next assignment to consider the intellectual and cultural dimensions of this phenomenon—to endeavor to understand the significance of power and law in the *mentalité collective*, both high and low, of the Russian people?

⁴⁸ For an interesting and suggestive illustration of the interaction between social and legal realities in the process of codification, see Adriano Cavanna, *La codificazione penale in Italia: Le origini lombarde*, Pubblicazioni dell'Istituto di storia del diritto italiano, Università degli studi di Milano, no. 3 (Milan: A. Giuffrè, 1975; pp. 317); and, for a comparative survey, see John P. Dawson, *The Oracles of Law*, University of Michigan Law School Publications (Ann Arbor: University of Michigan Press, 1968; pp. xix, 320). For the most recent efforts at coming to grips with the Russian situation, see A. H. Brown, "The Father of Russian Jurisprudence: The Legal Thought of S. E. Desnitskii," in William E. Butler, ed., *Russian Law: Historical and Political Perspectives* (Leiden: A. W. Sijkhoff, 1977; pp. 266), 117-41; William G. Wagner, "Legislative Reform of Inheritance in Russia, 1861-1914," in *ibid.*, 143-78; and René Beerman, "Prerevolutionary Russian Peasant Laws," in *ibid.*, 179-92.

Reviews of Books

GENERAL

S. N. EISENSTADT. *Revolution and the Transformation of Societies: A Comparative Study of Civilizations*. New York: Free Press. 1978. Pp. xvi, 348. \$15.95.

One of my great-uncles used to delight his grand-nephews with a machine called a Marbelator. It looked like a miniature steel-framed building under construction. You put a small marble in the slot at the top, and the ball began its antic trip through the machine. It clicked and whirred across bridges, down steps, and around corners, sometimes speeding and sometimes dawdling, often veering into the depths of the runways only to shoot out unexpectedly at a lower level. But when it rolled from the bottom chute, the marble was still the same glass ball that went in. *Revolution and the Transformation of Societies* is an abstract Marbelator—a simple, familiar argument careening through a vast conceptual contraption.

S. N. Eisenstadt sets out to elaborate a general theory of the conditions under which revolutions occur. He begins with the principle that conflict is inherent in all societies because the dominant cultural orientations inevitably conflict with some individual needs and with the interests of some disadvantaged groups; the greater the conflict, the greater the potential for some sort of protest or rebellion. Protest and rebellion, however, only become revolution under exceptional circumstances. (Although Eisenstadt avoids defining his crucial term “modern revolution,” he apparently has in mind a forcible transfer of political power, accompanied by extensive social change in the name of a plan for a new society.) Conflict only takes a revolutionary form, he argues, in societies (1) that are making a rapid transition from “traditional” conditions to an “open” system of political legitimation, an industrial economy, and a strong involvement in various international structures; (2) in which “traditional centers and groups” consequently face problems that overwhelm the existing arrangements for access to power; and (3) in which growing socioeconomic differentiation pro-

duces both mobilizable groups of aggrieved people and disaffected elites to link and lead them. That much is simply one more variant of the mass-society theories that were once popular explanations of fascism and other twentieth-century movements and that were in turn derivatives of the nineteenth-century line of thought (typified by Emile Durkheim) that treated conflict as a consequence of a poor fit between a rapidly advancing division of labor and a weakened common consciousness. Where such arguments are not tautological, they are generally wrong; they imply, for example, that displaced masses make revolutions; that rapidly industrializing countries are more revolutionary than others, and so on.

Eisenstadt overlays the tired old argument with elaborate concepts, typologies, and historical illustrations but offers neither a systematic body of evidence nor a sustained analysis of any particular revolution. Instead, abstractions tumble over abstractions: “Developments in the symbolic field,” runs a typical passage, “tended to result in the challenging of the bases of legitimation of the social and political order” (p. 57). What “developments”? Whose challenge? We never really learn. Instead, we hear about “segregative, coalescent and convergent patterns of change,” about imperial, imperial-feudal, modern, and neopatrimonial societies. On the rare occasions when the historical accounts are sufficiently concrete to permit their comparison with current historiography, they are frequently odd or even defective. For example, Eisenstadt claims that the “French uprising of 1848, the Paris Commune and the German upheavals of 1918” illustrate “the growing dissociation between extreme movements of protest and the more central political struggle” (p. 318). Unless the passage simply means that the three revolutions ultimately failed, it strangely ignores their central locations, their challenges to the existing structures of power, and their intimate connections with bourgeois and working-class politics both before and after the rebellions. Despite a broad, multilingual bibliog-

raphy, despite an occasional thought-provoking passage (such as the eight-page sketch of Jewish revolutionary traditions), despite an obsessive effort to deal with each apparent difficulty in the argument by introducing a new variable or a new distinction, despite endless clicking and whirring, the book's final product is a tiny, clouded crystal ball.

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BARRINGTON MOORE, JR. *Injustice: The Social Bases of Obedience and Revolt*. White Plains, N.Y.: M. E. Sharpe. 1978. Pp. xviii, 540. \$17.50.

Why are sociologists so much bolder than historians? Why do they ask questions that are more difficult and tackle problems that are more complex? Are they more knowledgeable or more naive? Do they have greater courage or just greater *chutzpah*? Such are the musings inspired by a reading of Barrington Moore's latest book on injustice or, more accurately, on the reasons why some men acquiesce in a form of authority that condemns them to oppression or even destruction, while others rebel against overwhelming odds. The very sweep of such an inquiry suggests a self-confidence that most historians lack. Perhaps they are too timid, perhaps too disillusioned, perhaps too wise. In any case, they must not overlook the work of a daring scholar in a sister discipline who has rushed in where they have feared to tread.

The author examines a number of social situations in which large groups have allowed themselves to be subjected to extreme mental or physical anguish. He looks at ascetics, for whom self-inflicted pain provides a source of spiritual satisfaction. Then come the untouchables of India, who are the victims of a system of values that they deplore and yet accept. And finally, there are those, like the inmates of the German concentration camps, who are exposed to torture and death for reasons they can neither justify nor understand. This last example in particular provides a model of what happens in society at large, displaying "the same class hierarchy, the same competition for crumbs among individuals in the lower strata, the same emergence of a reformist and arrogant elite among those in principle opposed to the regime, and a variety of mechanisms that produce in the subordinate strata an acceptance of the values of the rulers" (p. 76). Most of us live in an attenuated Dachau or Auschwitz.

The major part of the book, however, running to about 250 pages, is a detailed examination of the German working class between 1848 and 1920.

This is not really a systematic history of labor in Central Europe, but a study of its behavior during certain critical situations in its development. There is first a chapter on the workers in the Revolution of 1848. Then comes a section describing the outlook and mentality of industrial wage earners in the Wilhelmine period, especially the coal miners and the iron and steelworkers. Finally, there is an analytical description of the developments of 1918-20, the reformist revolution and the radical thrust, and the reasons why the political upheaval did not lead to a social reconstruction. The book concludes with a comparison of the Russian and the German revolutions, a discussion of whether events in Central Europe could have turned out differently, and lengthy excursions on the Nazi experience, moral relativism, and the sense of injustice.

The book as a whole is not so much an organized narrative as a succession of interconnected essays on injustice in history, with an almost Teutonic preoccupation with definition and an easy casualness about stylistic discipline. It is essentially a collection of meditations, reflections, insights, intuitions, hunches, and speculations presented by a sharp and sensitive mind. Most readers will be impressed by the wealth of information and by the skill with which the author uses it to make original comparisons. But a small minority—and I am afraid I belong to it—will find something self-indulgent about a lengthy work that winds and meanders while the writer offers his perceptions, which are sometimes sharp and sometimes—if the unvarnished truth be told—pedestrian. As for the question of why some men accept and others reject injustice, I do not know much more about that now than I did before reading the book.

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JÜRGEN KOCKA. *Sozialgeschichte: Begriff, Entwicklung, Probleme*. Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 173. DM 15.80.

This little volume consists of revised versions of three previously published essays on Marx's and Weber's methodology, on the concept of social history and its development, and on the social function of history. Perhaps more than any other West German historian of this generation, Jürgen Kocka has consciously sought to write a history guided by explicit theoretical presuppositions, derived in large part from Marx and Weber. In a sense, the three essays are a critical examination and justification of Kocka's own working assumptions. At the same time, Kocka sees conceptual

problems in Marx's and in Weber's methodology. Modern social history can heuristically hardly do without Marx; yet Marx, by his assumption that the student of society can actually grasp the substance of history understood as a totality, contains the elements of methodological and political dogmatism that Kocka wishes to avoid. Weber, in contrast, by supposedly viewing social reality as chaos, a "heterogeneous continuum" given meaning (*Sinn*) only through the conscious constructs of the social scientist, carries within himself the seeds of "decisionism" and ethical nihilism. This contrast is perhaps overdone, as Kocka recognizes, since Marx was very much aware that a historical object is not given, but results from the interaction of man-made subject matter and the active observer, while the Weberian ideal types, according to which reality can never be grasped in its totality, nevertheless presuppose that society is sufficiently structured to permit a comparison of concepts and reality. Kocka wishes to find a middle way between Marx and Weber, one that permits pluralism of approach and conceptualization and recognizes the role of extrascientific factors that enter into social scientific inquiry yet seeks to discover "criteria of objectivity," which make "rational discourse" possible and prevent political distortion (p. 45). For Weber, of course, the basis of objectivity was to be found in method and logic rather than in subject matter. Kocka in his search for standards of objectivity is unable to go far beyond these.

The attempt to mediate, this time between traditional narrative political history and modern structural history is continued in the second essay. Kocka sees the limits of "social scientific history," which reduces historical phenomena to quantitative functions and sees in an application of ideal types, indebted to Weber, the possibility of combining the analytical method of the systematic social sciences with a hermeneutic (*Verstehen*) approach that takes into account not only regularities and structures but also the extent to which human beings themselves influence the fabric of history.

In a sense, as becomes particularly apparent in the third essay, the book is a very personal, intelligently argued statement of belief that reflects the liberality and social seriousness of a good deal of the historical writing of the younger group of West German historians. Historical studies, Kocka insists, have always had political implications and have been particularly susceptible to politicization when historians have refused to recognize this. Kocka consciously opts for the enlightened values of "critical rationality" (p. 121) based on open discussion in the service of a more humane and democratic social order (p. 122). For the histo-

rian in the English-speaking world, the dialogue with Marx and Weber may loom overly large in a discussion of modern social history. Yet the problems raised in this thoughtful little volume certainly need to be addressed by social historians coming from very different traditions outside of Germany, including Marxists and those of a more positivistic social science orientation.

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W. W. ROSTOW. *The World Economy: History and Prospect*. Austin: University of Texas Press. 1978. Pp. xliii, 833. \$34.50.

Walt W. Rostow is one of America's most prolific scholarly authors. Trained in economics, committed professionally at an early age to the economic history of modern Britain, he has also written extensively on modern diplomacy and politics. It is unlikely that more than a very few people outside his immediate family are even aware of, much less familiar with, his vast output, consisting of at least seventeen books, not including revised editions and numerous scholarly and popular articles. He even published three books during his years as a high official of the Kennedy and Johnson administrations, a feat that deserves recognition in the *Guinness Book of World Records*.

The volume under review is perhaps Rostow's most ambitious effort to date. It includes an account of the origin of what is elsewhere called modern economic growth in late eighteenth-century Britain, its diffusion throughout the world in the nineteenth and twentieth centuries, its connection with the recent problems of worldwide inflation, population pressure, energy shortage, and environmental pollution, and it offers both a prognostication and a policy program to avoid global disaster. In both method and substance, Rostow draws heavily on his earlier works, notably *The Growth and Fluctuation of the British Economy, 1790-1850* (1952), *The Process of Economic Growth* (1952), and *The Stages of Economic Growth* (1960), but he has also accumulated a vast amount of information from official sources and from recent monographic literature.

Rostow is prolix as well as prolific. In addition to its own 833 pages, *The World Economy* has spawned two offshoots that were originally intended to fit within its covers: *How It All Began* (1975), a more extended account of the origins of modern economic growth, and *Getting from Here to There* (1978), a more detailed and technical exposition of his policy proposals. The book itself is

organized into fifty-five chapters in six parts, supported by four appendixes and almost one hundred pages of notes in small type; it is replete with tables and charts, even in the notes. All in all, it is a monumental production, at least from the book-maker's point of view.

The book begins conventionally enough with four chapters devoted to "The Demographic Transition" in part one. Part two (five chapters) presents "Growth since the Eighteenth Century: An Overall View." The analysis commences in earnest with part three (ten chapters), "Trend Periods." Rostow's trend periods, also known as long waves, long swings, and secular movements, consist of periods varying in length from fifteen to forty-seven years during which certain economic time series exhibit—or appear to exhibit—uniform trends. These trends, mainly in prices, were observed for the nineteenth century by the Russian economist N. D. Kondratieff and figured prominently in Joseph A. Schumpeter's *Business Cycles* (1939); most economists no longer regard these long swings as significant for other than taxonomic purposes, but Rostow professes to see in them the key to understanding the future evolution of the world economy.

Part four (seven chapters) continues the analysis, focusing on business cycles. Parts three and four purport to be global in scope, although in fact the basic data underlying the analysis derive overwhelmingly from Great Britain and the United States. In part five (twenty-two chapters) Rostow turns to individual countries and gives potted economic histories of twenty of them within his stages-of-growth framework; those selected range from the industrial powers of Europe and North America to Iran, India, China, Taiwan, and Thailand. Finally, in part six (seven chapters) Rostow tackles the limits to growth and related issues and gives in summary form his prescription for a smooth transition to an indefinite period of worldwide high mass consumption.

This is not the place to enter into a detailed technical critique of the book. It is certain to provoke both extravagant praise and savage condemnation (and that from a variety of viewpoints). In my opinion it merits neither. Strictly from the viewpoint of data collection and exposition, it represents an imposing feat for a single scholar, even when aided by teams of research assistants. Moreover, much of the historical analysis is sound, and many of the policy recommendations are eminently sensible. As a comprehensive, unified interpretation or explanation of the past, present, and future of the world economy, however, which is what it purports to be, it is far from convincing.

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CHARLES P. KINDLEBERGER. *Economic Response: Comparative Studies in Trade, Finance, and Growth*. Cambridge: Harvard University Press. 1978. Pp. viii, 308. \$16.50.

For some time historians have been the target of friendly and not so friendly attacks from an increasingly large number of cliometricians. They will be relieved to learn, however, that this collection of essays by one of the profession's most respected economists is not yet another assault on their craft. On the contrary, in Charles P. Kindleberger historians will find a strong and articulately, which is apparent from the author's remarkable closing statement: "It is my considered opinion, as well as my passionate conviction, that economics needs history—perhaps even more than history needs economics" (p. 241).

The cliometric revolution has so far not unfolded in this spirit, a point made both by Kindleberger and by Donald McCloskey in his recent article "Does the Past Have Useful Economics?" (*Journal of Economic Literature*, 14 [1976]: 434–61). The relationship between economics and history has been rather one-sided with economists seeking to demonstrate the value of economic models and statistical techniques for historical research. In contrast, the impact of cliometrics on economic theory and policy has been negligible. Kindleberger's description of himself—"a literary economist who is more historical economist than economic historian, interested in testing economic models against historical fact more than in using particular economic models to understand history" (p. 223)—suggests that he is a scholar who can perhaps reverse this trend.

In these essays Kindleberger extols the virtues of comparative economic history, which he argues "allows us to test a given economic model for its generality and its specificity by examining the same process or stimulus under other conditions" (p. 2). The first and last chapters are devoted to an elaboration of this theme along with a brief review of existing work in comparative economic history and an agenda for future research. An excellent bibliography appears at the end of the book.

The remaining chapters draw together separate pieces of Kindleberger's research that explicitly demonstrate his comparative approach to economic history. In each case the author stresses the relevance of his research for economic theory or policy. Chapters two, three, and six point out the limitations of economic theory as a tool of analysis. "Group Behavior and International Trade" is a study of the different responses of the United Kingdom, Germany, France, Italy, and Denmark to the world decline in the price of wheat after 1870. Kindleberger argues that a proper under-

standing of these responses awaits a nonmarket explanation of group behavior, since the differences cannot be accounted for by economic theory alone. Similarly, in "The Rise of Free Trade in Western Europe, 1820-1875" he finds the traditional theoretical literature on commercial and trade policy an incomplete guide for understanding the nineteenth-century experience. He concludes that "the movement to free trade . . . suggests the possibility that Europe as a whole was motivated more by ideological considerations rather than economic interests" (p. 65). Finally, in a previously unpublished lecture entitled "European Port Cities" Kindleberger stresses the inadequacies of location theory in explaining the rise of European port cities, their relationship with one another, and with their hinterland.

In chapters four and seven the author turns from theoretical issues to matters of economic policy. In "The Formation of Financial Centers" he arrives at generalizations about the evolution of financial centers in nineteenth- and twentieth-century Europe and North America. From these generalizations he argues that Brussels is the most logical candidate to become the financial center for the European Economic Community, an event he foresees by the late 1980s. In chapter seven the rivalry between Germany and England for economic supremacy in pre-World War I Europe is discussed in the context of the phenomenal growth of the Japanese economy and the accompanying doubts about the economic future of the United States.

Chapter five is the only essay with no strong orientation toward economic theory or policy. In this stimulating application of the comparative method, Kindleberger finds only a loose relationship between the commercial revolution and the Industrial Revolution in Europe. In England, substantial commercial change seems to have preceded the eighteenth-century Industrial Revolution. By contrast, Holland experienced a commercial revolution but no industrial revolution, while Germany industrialized successfully with no obvious commercial transformation.

Specialists in each of the specific areas treated by Kindleberger in his comparative approach will no doubt wish to raise serious questions about his analysis. For example, more traditional economic historians will be uncomfortable with his reliance on secondary sources and his penchant for broad generalization. Cliometricians will be troubled by his reluctance to use formal economic models in his investigations. But these essays are in a sense powerful responses to such criticism. If a truly interdisciplinary link between economics and history is to develop, traditional historians must not hesitate to synthesize their vast monographic literature, while cliometricians must begin to treat

their models with healthy skepticism. Those who pursue these tasks will find much of value in these provocative essays.

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CHARLES P. KINDLEBERGER. *Manias, Panics, and Crashes: A History of Financial Crises*. New York: Basic Books. 1978. Pp. xii, 271. \$12.95.

In his previous book, *The World in Depression, 1929-1939* (1973), Charles P. Kindleberger explored international monetary policies during a time of crisis and deep depression. In his present study, he has greatly extended his inquiry to cover financial crises from the South Sea and Mississippi Bubbles in 1719-20 to the Great Crash of 1929, with forays before and after, right up to the current financial crisis in New York City. His approach to his subject is historical and comparative, and his organizational method is topical rather than chronological. That is, he analyzes financial crises by comparing them over time and across national boundaries, concentrating primarily on England, France, Germany, the Netherlands, and the United States, with occasional references to Italy, Russia, the Scandinavian countries, Canada, Latin America, and the Far East. The study, of necessity, is based on secondary sources, although the author also draws freely from English and European literature to portray the human condition as it exposes itself under duress.

The account is tightly structured, centering on a model that Kindleberger adapts from the work of the monetarist Hyman Minsky. Using this to establish the "anatomy of a typical crisis," he describes the progression of events and behavior that occurs at the peak of a business cycle turning downwards. These include "displacement," "overtrading," "monetary expansion," "euphoria," "revulsion," "discredit," and "panic"—terms which he clearly defines. The model then provides the basis for arranging the historical materials that make up the bulk of the work. This historical exposition, in turn, is meant to establish the economic validity of the model as a guide to monetary management.

Kindleberger, who describes his approach as "literary economics," personally identifies with economists in authority during the 1930s, before, as he remarks, this older view "unaccountably slipped into disrepute during the Keynesian revolution and the monetarist counterrevolution." He regards the newer schools of thought as "incomplete," and he makes his disagreements explicit throughout, particularly with Milton Friedman and his followers regarding the interpretation

of specific crises and their theoretical significance. His own position is that financial crises have become increasingly infectious internationally; and he opposes relying solely on either the self-adjusting mechanism of the market or putting one's trust in prescribed, automatic monetary adjustments to correct a crisis or to prevent panic. Monetary management, in short, is an art that depends finally on the knowledge, experience, timing, and the wisdom of the monetary authorities. Furthermore, the lesson of history here, which the author stresses repeatedly, is that this art must be exercised in times of financial crisis by someone willing and able to assume responsibility as "the lender of last resort"—whether by a central bank, the treasury, or an international body.

Although the theoretical implications of this study are clearly a matter for monetarists to settle, Kindleberger has provided historians with a lucid account of his own views and a window for understanding the economic counterpart of a historiographic debate.

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JÜRGEN KUCZYNSKI. *Zur Geschichte der Wirtschaftsgeschichtsschreibung*. (Studien zu einer Geschichte der Gesellschaftswissenschaften, number 8.) Berlin: Akademie-Verlag. 1978. Pp. 230. 15 M.

Jürgen Kuczynski, author of a forty-volume history of the working classes under capitalism and one of the most prominent East German economic historians, has been engaged since 1975 in producing a series of "studies" in the history of the social sciences, of which this volume is the eighth to appear. Kuczynski's approach to the subject is well established, and this is vintage Kuczynski, idiosyncratic and even precious in style, argumentative and contentious in form, and didactic in intent. More than a third of the text consists of unassimilated quotations, interspersed with commentary by Kuczynski. Heavy-handed irony for ideological enemies and lavish praise for friends alternates with special pleading and self-advertisement. Kuczynski takes the time to inform us that the first serious bibliography of early works on labor history appeared in volume twenty-six of his history of the working classes (but it remains "completely unsatisfactory" because he was refused a travel permit [p. 127]) and that he and his father between them have written "over 100 books and pamphlets and far more than 1000 articles" on the condition of the working class (p. 140).

Nonetheless, there is a serious book here. Based on his reading of Engels, Kuczynski asserts that every social science must be both theoretical and

historical in approach. Each discipline may be divided into specialties, but theory and history remain essentially inseparable. Therefore, the reader will need to consult volume one of the present series of studies, which deals with "political economy," together with the present volume on the writing of "economic history" because only together do these subfields make up the true discipline of "economics." This approach dictates the form of the present volume, which is not concerned so much with the writing of economic history in itself as with the origins and development of "economics" as a science combining historical observation with theoretical insight.

The most substantial chapter traces the development of stage theories of economic development, from seventeenth-century thinkers such as Bodin, Grotius, and Pufendorf through Enlightenment thinkers in both France and Scotland. Kuczynski places particular emphasis on Adam Smith, whose recognition of "a tiny bit of history, namely that the capitalist society which ruled for all eternity had been preceded by a primitive society," led immediately to "a mighty step forward in political economy . . . the clear distinction of the manner of appropriation and distribution of the social product in society based on exploitation and in the time before its existence" (p. 107). The culmination of this line of development is, of course, the emergence of Marxist historical materialism. Though Kuczynski is willing to concede that Marx's successors still have much to do, "the decay of bourgeois stage theories" from Hildebrand and Bücher to Rostow is "all the more astonishing" by comparison (p. 110).

In focusing on the Marxist form of historical materialism, however, Kuczynski actually fails to produce a history of its development. He offers no explanation of why some early economic thinkers combined theory with historical observation. Rather, he celebrates each of those thinkers who anticipated some aspect of Marx's historical materialism, without much concern for their thought as a whole or for their influence on contemporaries and successors. Thus, his reading of Smith will strike many as one-sided and his emphasis on Goguet and Millar disproportionate to their influence.

In addition, Kuczynski gives us only a portion of the history of economics under his own definition. He offers no history of the development of Marxist economics after Marx and no analysis of the process by which theoretical "economics" in a non-Marxist sense became separated from economic history. The chapters on the German historical school and on the twentieth century therefore lack a theoretical framework comparable to the earlier chapters and degenerate in places into a recitation

of names and titles. Many of his remarks are acute (for instance on the debate over the Asiatic mode of production, the *Annales* school, or the new economic history) but they remain isolated because of Kuczynski's failure to employ his own theoretical approach to better advantage.

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JOHN CRITCHLEY. *Feudalism*. Boston: George Allen and Unwin. 1978. Pp. 210. \$17.25.

John Critchley, a specialist in English feudalism, has traveled the highways and byways of world history, from the ancient Near East to twentieth-century Latin America and Asia, seeking examples of feudalism for this presentation, apparently intended for the general reader. He is, of course, correct in maintaining that shared ownership of land, tenure in return for military service, clientage, dependent peasants, and even vassalage were not unique to medieval Europe. In stressing the uniformities in social, political, and economic organization between preindustrial Europe and other societies, he expands several themes developed at the 1950 Princeton University Conference on Feudalism (edited by R. Coulborn as *Feudalism in History* [1956]).

Critchley's best chapters are the two grounded most closely on the medieval experience, on fief tenure and vassalage. His discussion of feudalism as a political concept is not successful, for it is not made clear why the term feudal must be employed to describe the processes of creation and disintegration of centralized states. Part of the problem is that medievalists have yet to determine satisfactorily what was feudal in the European monarchies and what role the state played in forging the distinctive combination of feudal elements that, Critchley claims, accounts for the later emergence of capitalistic society in the West but not in other societies containing only some feudal elements.

In spite of interesting comments throughout, this study suffers the weakness of all macrohistorical comparative approaches. Certainly medieval Europe was feudal in some sense, but simply to characterize it as a feudal society, which then serves as a basis of comparison for other societies, is to misrepresent its complexity and evolution. Not all land was feudal at any time, and not all of those who had fiefs possessed only fiefs. Moreover, the nature of feudalism—if one must use the term—in Europe was fundamentally different in the ninth, eleventh, and thirteenth centuries, as were the society and economy. Recent conceptual

contributions to the study of European feudalism as well as the great number of detailed local and regional studies on medieval society no longer permit the construction of vast syntheses or comparative models in the tradition of earlier general works on feudalism.

Critchley's enlargement of the concept of feudalism to its widest possible extent, to include all societies with a form of dependent military service, vassalage, or subject peasantry, robs the term of its meaning. So general and various does feudalism become that, like capitalism, it cannot serve as a useful analytic or descriptive concept. It is not clear what is achieved by cramming bits and pieces of information on hundreds of societies widely scattered in time and place into the worn mold of feudalism.

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DONALD WINCH. *Adam Smith's Politics: An Essay in Historiographic Revision*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1978. Pp. xi, 206. Cloth \$22.95, paper \$6.95.

Donald Winch offers an interesting and readable monograph on Adam Smith's political views, grounded in what he believes to be "a historical interpretation of Smith's intentions and achievements" rather than the liberal-capitalist perspective that Winch feels has dominated previous attempts at analyzing Smith's political attitudes. Rejecting the view that Smith's works mark the point in the development of liberal thought at which economics envelops and eventually eclipses politics, Winch's contention is that a careful reading of Smith's *oeuvre*, faithful to the context within which it was penned, indicates that Smith indeed possessed a coherent "politics." By emphasizing the discontinuities between Smith's political views, on the one hand, and classical economics and nineteenth-century liberalism, on the other, Winch has brought into sharper focus the more purely eighteenth-century elements that pervade Smith's thought, particularly his debt to Montesquieu, Francis Hutcheson, and Hume.

Winch has chosen to examine his subject by concentrating on Smith's treatment of three of the great political issues of the day: the question of standing armies, the public debt, and the events in America. In doing so, he has called attention to those aspects of Smith's thought that deal with the singular importance of a stable and smoothly functioning political order. Winch has done this by an almost scrupulous avoidance of Smith's econom-

ics. As a result, despite his efforts to construct an independent "politics" from Smith's analysis of these issues, nothing like a comprehensive political vision emerges. It is true that Smith's works issue from the school of Scottish moralists of which he was a member, and we are indebted to Winch for reminding us of this when we are tempted to read Smith as if he were a nineteenth-century economist. But any attempt to study the Smithian corpus by purging it of its economic statements is doomed to fall short. If it is erroneous to read Smith's economics as the sole indicator of his political views, it surely is equally futile to offer a theory of politics devoid of reference to Smith's conclusions regarding the proper relation between government and the marketplace.

It is particularly disappointing that Winch has so hastily dismissed the concept of spontaneously generated orders—whereby complex social arrangements are the product of human action but not of human design—and the role this notion plays in Smith's approach. Although the concept is most visible in Smith's treatment of economic phenomena, in which he coined the term "invisible hand" to describe it, there is ample evidence that Smith was aware of its implications in explaining the nature of a multiplicity of social and political institutions. Nor was he alone among the Scottish moralists in employing the notion. As Friedrich Hayek and Louis Schneider, among others, have pointed out, the relation between individual actions and unintended but ordered social outcomes emerges in the writings of Hume, Adam Ferguson, Dugald Stewart, and, to a lesser degree, in Thomas Reid and Lord Monboddo. Given the crucial role this concept played in Scottish Enlightenment thinking, the reader is at a loss to understand why the author summarily excludes it when considering Smith's perspective on political matters.

Winch's zeal to search out the political dimensions of Smith's thought, at the cost of rejecting all previous scholarship that has placed Smith at some point on a continuum from Locke to nineteenth-century liberalism, has led him to misconstrue the general tenor of Smith's philosophy. To claim, as Winch does, that "Smith did not advocate the establishment of a particular economic order called capitalism" (p. 180) is, at best, misleading. No analysis of Smith can justifiably fail to take account of the facts that Smith indeed presented the first systematic account of the laws governing the production and distribution of wealth in a free market and that, despite the political, social, and ethical milieu of Scottish moral philosophy from which his economic writings emerge, there is no denying the pivotal position he held in the development of liberal thought in the

century following. It is true that Smith's connections with the philosophical positions of Hutcheson, Hume, and Montesquieu were close. But it is no less the case that, judged from a slightly altered perspective, these connections were as intimate with Malthus, Ricardo, and the other classical economists who followed him.

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HERBERT S. KLEIN. *The Middle Passage: Comparative Studies in the Atlantic Slave Trade*. Princeton: Princeton University Press. 1978. Pp. xxiii, 282. Cloth \$20.00, paper \$9.75.

Herbert S. Klein's newest work on the slave trade is a welcome addition to the literature, and specialists will find that each of the volume's ten essays sheds light on some previously obscure corners of that trade. The book is particularly valuable in its attention to the non-English participants in the "Middle Passage," and only two chapters, one on the trade to Virginia and another on Jamaica, deal with the staple fare of Anglo-American trade.

The book is heavily quantitative, with nearly ninety tables and graphs covering such topics as slave shipments, slave origins, landings at selected ports, age and sex data, sailing characteristics of slave ships, details of tonnage, slaves per ton, frequency of voyages, and the like. The quantification does not at any point detract from readability, however, and Klein's analysis does not go beyond the use of simple statistical tools.

An introductory chapter on the American demand for slaves is followed by a discussion of the Portuguese trade from Angola; the trade to Rio de Janeiro, including the shipping patterns and mortality on that run; Brazil's internal trade; the chapters on Virginia and Jamaica mentioned above; an analysis of the eighteenth-century French slave trade; and the Cuban trade between 1790 and 1843. Generalists will find the concluding chapter most interesting, because it highlights the common threads that weave together these various accounts of the slave trade. The conclusion emphasizes a rather surprising similarity in passage times, in the conditions of housing and feeding slaves in transit, and—especially noteworthy—a tendency to uniformity in the tonnage displacement of individual slave ships in the eighteenth and nineteenth centuries, which Klein concludes was an arrival at an "optimal size" for the vessels in the trade. The conclusions on mortality during the passage are of equal interest. Klein continues his earlier work on whether cargo density—"loose packing" or "tight packing" in the

familiar jargon—had an effect on death rates. In spite of the age-old assumption that crowding did have a detrimental effect, Klein finds no significant correlation between the number of slaves carried per ship and mortality rates. He shows instead that mortality was more closely correlated to time spent at sea on the voyage, and the resulting longer exposure to dysentery, “the major systematic killer that seemed to have affected almost all voyages” (p. 234). Klein calls for further coordinated research to determine the major causes of passage mortality, but the book effectively explodes the unicausal “tight packing” explanation for high death rates on the Atlantic passage.

Other interesting conclusions concern the curiously high numbers carried per ton from the Loango and Angolan coasts in comparison with West Africa; the marked seasonal variation in slave shipments, which appears linked to the annual cycle of the growing season in the Americas; and the consistent predominance of male adults over women and children in the trade, explained by the retention of females in the African agricultural labor force and the economic penalty paid in the form of low sale prices for children.

Klein’s research is thorough, and his bibliography is up-to-date and comprehensive. His lengthy list of unpublished papers includes many of the latest works; his manuscript materials draw on archives off the normal beaten track to London, such as those at Nantes, Seville, Lisbon, Madrid, and Rio de Janeiro; and he has made innovative use of Rio de Janeiro newspapers of the nineteenth century in his analysis. Add to this a handsome job of publication by the Princeton University Press and a good index, and the book becomes a valuable reference for specialists and an item that must be in any library claiming to cover the era of slavery and the slave trade.

JAN HOGENDORN
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J. M. POWELL. *Mirrors of the New World: Images and Image-Makers in the Settlement Process*. (Studies in Historical Geography.) Hamden, Conn.: Archon Books. 1977. Pp. 207. \$15.00.

Geographers disagree on the definition of historical geography. For some, historical geography means the reconstruction of previous landscapes. For others, historical geography is broader in meaning and encompasses the physical and human geography of historical periods. Apparently R. H. Baker and J. B. Harley, the editors of *Studies in Historical Geography*, subscribe to this tolerant definition by including J. M. Powell’s *Mirrors of the New World* in this series. Powell, however, states

that, although his work is historically oriented, “it is not an ‘historical geography’ in any commonly accepted sense” (p. 13). Instead, the book is intended primarily for students of human geography with the purpose of outlining “the characteristics and potential of one type of humanistic perspective in geography” (p. 10), that perspective being of images and image-makers. In addition, Powell views his book “as a brief exploration in one type of *dialectical* human geography” (p. 26). Thereby Powell hopes to encourage “the students of today who seem to feel the need for engaging in independent exploration more urgently than most of their predecessors” (p. 174).

Leaving aside the issue of whether Powell has written upon historical geography, what has he tried to do? Basically, he has attempted to demonstrate the value of human attitudes, especially “hopes, fears and aspirations” as “a necessary balance to more orthodox explanations of geographical change” (p. 174). Powell’s subject is settlement activity in particular areas of North America, Australia, and New Zealand and the influence of certain ideas and values upon settlement and the resulting images. He explores the fundamental bond between land and society in Europe and its bearing upon emigration and settlement in the New World. Powell is quite intrigued by the ideal of the “yeoman farmer” and offers examples of its influence not only on promotion of settlement but even upon legislation such as the Homestead Act of 1862 in the United States. Nineteenth-century Australia offers several illustrations of what Powell calls the “quest for Arcady.” Also considered are attitudes of settlers toward native inhabitants, the changing image of the wilderness, and settlers’ misconceptions of certain physical environments. Another idea treated by Powell is “Elysium,” or the search for health, with California and Australia providing illustrations of useful but not identical images. Finally, Powell considers examples of utopias, millennial thought, and cooperatives as offered by some New World experiments. The topics, then, are numerous, the structure loose, all held together by a permissive title.

Although geographical and historical writing ideally should contain both description and explanation, Powell’s attempt to further his type of explanation in geography is both dull and unimpressive. The early chapters are awash in psychological and sociological jargon. He is unsuccessful in his effort as a social scientist to blend sociology, psychology, and communication research with history and geography. A more serious defect is the author’s failure to discriminate between important and trifling ideas in history and geography. Thus Powell can conclude: “The very humanity of our natures makes mental travellers of us all, and this is probably reason enough for the pursuit of any

type of cultural-historical perspective" (p. 175). Indeed, one may declare all perspectives are equal, but some perspectives are more equal than others.

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MASSIMO SALVADORI. *The Liberal Heresy: Origins and Historical Development*. New York: St. Martin's Press. 1977. Pp. xv, 248. \$18.95.

The historiographical literature on liberalism and its problems during the past century is relatively scarce. One reason for this is the problem of definition. One knows what a socialist, a communist, or a fascist is because they all created movements. But liberals are more elusive: they cannot be identified with, or limited to, the parties that bear the name.

Yet there is a liberal tradition, whose continuing importance is attested to by the fact that the two distinctive political movements of the twentieth century, communism and fascism, originally defined themselves in terms of their opposition to liberal values and institutions. Massimo Salvadori has set out to describe this tradition, to summarize its historical development, and to place it in the context of contemporary political and ideological trends. A sequel, he promises, will deal with liberalism's strengths and weaknesses, its achievements and failures, and its prospects for the future.

Salvadori begins with the problem of definition. The central concept of liberalism as a movement, he claims, is a commitment to "the equal liberty of all." Historically, the liberties liberals fought hardest to achieve and to preserve have been freedom of expression and the right to select (and to remove) their political representatives. Salvadori believes that these two liberties taken together provide the only criterion for distinguishing a free society from an authoritarian one. Economic liberty, while historically important to liberals, is not essential to the liberal program, for "liberal institutions are concerned with the way things are done more than with what is done" (p. 49). Indeed, Salvadori insists that one of the differences between the liberal and the authoritarian is that the liberal maintains a commitment to diversity and refuses to dictate any fixed future for mankind.

According to Salvadori, the liberal program had its origins in a series of events well known to students of Western civilization, of which the most important were the Greeks' discovery of reason, the Romans' secularization of law and politics, the nominalists' defense of individualism within the medieval Church, the humanists' commitment to the dignity of man, and the Protestants' belief that the individual was directly responsible to God.

These ideas crystallized in the second half of the seventeenth century into the Whig program and, after the revolution of 1688, spread throughout the English-speaking world. A more democratic American variant of these ideas became extremely influential after 1776. On the Continent liberalism was given a different interpretation by the course of the French Revolution. Critical to the later history of liberalism was the appearance of the Jacobins, whom Salvadori places outside the liberal tradition, as he does conservatives who try to obstruct change, anarchists who refuse to recognize any limits to their freedom, and socialists who give priority to the collectivity over the individual.

Salvadori stresses the fragile, precarious, and extremely limited nature of the liberal success during the three centuries since the Glorious Revolution. Liberalism, he says, took firm root only in the English-speaking world and in a few Western European countries. Even there, resistance to the liberal program remained strong, especially among intellectuals. Today the dominant trend in the world is set by the scores of authoritarian states that combine "fanatical nationalism" with "dogmatic socialism." Hence the title of the book: for Salvadori, liberalism represents a heresy within the authoritarian orthodoxy that envelops in its gloomy mist almost all of human history. Few human societies have cared to become free; fewer still have known how to remain free. Salvadori sees little reason to hope that contemporary societies will constitute an exception to this depressing rule.

Salvadori's impassioned defense of liberal values commands admiration and respect. But, alas, while sometimes plausible as a polemic and while commendably ambitious in its global scope, *The Liberal Heresy* is disappointing as a work of history. It dwells too much on what liberals said they believed—or better, on what Salvadori thinks they should have believed—rather than on what they actually believed and on what they did once in power. Nor does it deal adequately with the circumstances that led to liberalism's crisis and the rise of antiliberal political movements in the twentieth century. Presumably, Salvadori's second volume will deal with these critical issues. One hopes so, for the task of the historian of liberalism is not to reformulate the aspirations of liberalism's founders or to define its unchanging essence but to explain why its apparent ascendancy in the nineteenth century was checked and reversed in the twentieth. Surely, it is not enough to argue, as Salvadori does, that most men are unable to bear the burden of freedom and unwilling to live with the uncertainty created by the necessity of individual choice.

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GIANFRANCO POGGI. *The Development of the Modern State: A Sociological Introduction*. Stanford: Stanford University Press. 1978. Pp. xiii, 175. \$10.95.

Gianfranco Poggi, professor of sociology at the University of Liverpool, tells us that members of his profession have of late become more interested in problems concerning the state largely because of the state's increasing involvement in societal affairs. To better understand the state, he has chosen to rely on "the history of Western political institutions and, to a lesser extent, constitutional law" (p. xi). Other disciplines, he suggests, might have something to contribute, but he has ignored anthropology because it is "boring," economics because he does not "understand" it, and political science because during the last thirty years it has gone "to incredible lengths in order to *forget* the state . . ." (p. xiii). Readers of the *AHR* should be pleased to find their discipline preferred, but we are not all equally treated, for Poggi tells us that he relies primarily on publications in German because they are more frequently written "in general terms and from a comparative perspective . . ." (p. xi).

After thus disarming his readers, Poggi discusses the nature of politics and then embarks on a history of the state during the past thousand years. Twenty short pages are sufficient to explain the origins, nature, and evolution of feudalism and twenty-four more are assigned to the *Ständestaat*, which he quite properly interposes between the feudal and the absolutist state. After treating the absolute state, he devotes a chapter to the nineteenth-century constitutional state. In these four chapters he concentrates on Germany and France. England he largely excludes because "particularly after feudalism, [it] does not fit easily into the argument, even in the highly abstract terms in which I phrase it" (p. 17). In a final chapter on the contemporary state he points out that "the institutional differentiation between socio-cultural and economic processes on the one hand and political processes on the other, which was characteristic of the West in the nineteenth century, has largely ceased to operate in our own" (p. 121). This invasion of the state into social and economic matters, characteristic of communist as well as capitalist societies, seeks legitimacy by helping to produce "an ever-increasing flow of goods and services for the consumer" (p. 134). Poggi regrets this development because he finds that the apparatus of the state has escaped from the control of parliaments, "the welfare system" has failed, and political dissent "manifests itself frequently in unconstitutional and sometimes criminal forms . . ." (p. 145). Hence, he urges the state to seek legitimacy by returning to "the liberal idea of the rule

of law, and the democratic idea of the participation of the ruled in the process of rule" (pp. 147-48).

There is little in this book for historians, and sociologists deserve a more balanced and detailed study. Nevertheless, Poggi deserves credit for his clear, well-organized presentation. He uses many of the leading Italian, French, English, and American authorities as well as those of Germany. On the whole, he displays good judgment in his highly personalized account, although, of course, one's opinion of the final chapter on the contemporary scene is likely to depend on one's politics.

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ROBERT G. WESSON. *State Systems: International Pluralism, Politics, and Culture*. New York: Free Press. 1978. Pp. viii, 296. \$14.95.

All historiography is skewed by class allegiance, but most of the history written by bourgeois scholars strives mightily to save appearances. Such is not the case with Robert G. Wesson, the Santa Barbara political scientist, who never leaves his readers guessing what banner he so proudly hails. His unsophistication is almost shocking, as he no doubt hopes it will be. In an age of cynical relativists and self-flagellating leftists, Wesson comes forward to proclaim the simple virtues of commercial competition, the rivalry of sovereign states, and good old-fashioned material progress.

State Systems is actually the second volume of a general survey of world history that began with *The Imperial Order* in 1967. In the earlier work, Wesson undertook a massive comparative study of the empires of history, cataloguing their glories and tracing their inevitable decay. Now he takes a step backward to analyze the robust international systems from which most of these empires evolved. A state system, in his definition, is "a group of closely interacting and therefore competing sovereignties for whom interrelations are comparable in importance to domestic affairs" (p. 11). His chief example is Europe between the Renaissance and the First World War. Others are medieval Europe, the city-state system of ancient Greece, the kingdoms of pre-imperial China and India, and Sumeria in the third millennium B.C.

Taken together, the two volumes present a complete cyclical theory of world history. During the age of the state system, a culture thrives. Inventors invent, traders trade, and everyone profits from the rude pushing and jostling of rival states. Although aristocrats may hold the reins of power, it is the middle classes who make the difference in this age of freedom and initiative. "The pluralism of a pri-

vate enterprise economy is of prime importance for political pluralism. Fractionated control of the economy is a basis for fractionated political power" (p. 7).

But the aftermath of the state system is the imperial order. Power centralizes and freedom withers. Like Spengler and Toynbee before him and all the theorists of historical cycles going back to Thucydides, Wesson revels in the virile spring-time of culture. Like many of the others, he finds his own society in transition from a rugged but now unhappily obsolete pluralism to a decadent imperialism. Yet he nurses the hope that a miracle of reason may somehow spare modern civilization from its predictable absorption into a world empire probably ruled from Moscow, the Third Rome.

Wesson deserves applause, as always, for his ambition. But ambition is not matched by performance. His sources are a curious jumble of venerable sages, college textbooks, and scholars of all qualities and generations. A book published in 1930 is characterized as "recent." Facts are carefully selected to prove the same point over and over again or twisted to fit. Major authorities are ignored, as are most of Wesson's predecessors and contemporaries in the field of comparative world history.

The outcome is a singularly flat, superficial, and uninspired work. It reminds one of a typical Soviet textbook with all the values turned upside down. Wesson might have extracted a pretty fair polemical piece from his research, but as a full-sized adventure in cosmic scholarship, *State Systems* does not succeed.

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FRIEDRICH V. KRATOCHWIL. *International Order and Foreign Policy: A Theoretical Sketch of Post-War International Politics*. Preface by RICHARD FALK. (Westview Replica Edition.) Boulder, Colo.: Westview Press. 1978. Pp. xix, 298. \$20.00.

In this thought-provoking work, Friedrich V. Kratochwil asserts that international order is influenced by certain "inference guidance devices"—or, as he sometimes calls them, "rules of the game." The most important of these in his estimation are laws (which he chooses to spend little time discussing), historical analogies, myths, metaphors, and doctrines. Human beings are rule-governed, maintains Kratochwil. So when acknowledged by all sides in diplomatic exchanges, rules create order by setting parameters for actions, serving as background knowledge in determining a

reasonable policy, and providing signals that make the intentions of competing nations clearer.

Much of this book could more easily be categorized as political science than history. In the first part, the author systematically expounds his theory. Then in the second section, he relies on history as empirical proof of this analysis. He concentrates on the years from 1945 to 1972. With 1945 came the end of World War II and the breakdown of the traditional European state system, leaving the Soviet Union and United States to establish a new international order with their own rules. Richard Nixon's visits to China and the Soviet Union designate the end of this study, for to Kratochwil these trips symbolize the impending demise of the Cold War order that the two super powers had fabricated. This date also underlines another facet of his theory. International order is not static, but changes, especially as new factors intrude. As the Cold War moved into the 1960s, the strategic considerations of atomic weaponry and the encroachment of the Third World as a new force in the previously bipolar international structure gradually compelled a realignment of the established international order.

Kratochwil believes that a recognition of these rules of the game allows nations to more easily understand each other's motivations and therefore creates stability. But despite this apparently idealistic contention, he is a realist. He does not see solutions in a drastic alteration of existing diplomatic conditions, but his theory attempts to adapt to the extant international environment. In the conclusion, furthermore, he recognizes that as the assumed knowledge resulting from these devices becomes more established the possibility of false signals presents a danger as nations either intentionally or accidentally avoid the rules, thus disrupting the stability of the diplomatic game.

Relying more on secondary sources, memoirs, and speeches than research in government archives, Kratochwil adds nothing new to knowledge of the Cold War per se. Two flaws mar the work. His style is choppy and hard to follow. In addition, the book lacks close editing. Typographical errors and misspellings abound. Yet overall he succeeds in placing the international situation of the Cold War in a new perspective.

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A. P. THORNTON. *Imperialism in the Twentieth Century*. Minneapolis: University of Minnesota Press. 1977. Pp. xii, 363. \$20.00.

In this highly interesting and thought-provoking book, A. P. Thornton of the University of Toronto

examines imperial policies and implementation, the self-image of the rulers, the views of their critics, and the protests and governing record of their nationalist successors, concluding that the more things change, the more they stay the same. Noting that the story is complex, he holds that it can be clarified by looking at policy. The works he cites in notes include polemics, monographs, theoretical tracts, and biographies. Many are written at a rather high level of generalization, as is much of this essay itself. One admires the author's courage.

There is much to agree with: political independence does not necessarily bring the economic kind in its wake; there was more freedom in some colonies than in European dictatorships of the twenties and thirties or than is found in Russia today; judgments should be made on actions, not on words; in their campaigns for self-government, nationalists made good use of both the ideas of their erstwhile masters and the comparative liberalism of their regimes. On the other hand, hardened infantrymen of colonial research may raise questions here and there: the white men's clubs and the attitudes of civil servants toward them, as described in the first chapter, will not be recognized by all of the old boys; Lyautey is called the century's most successful paternalist despite the fact that he came after Lugard, to whom he acknowledged great debts; peasants are said to have remained unchanged; the reaction of the Colonial Office to Sir Charles Mitchell's recruitment criteria was exactly the reverse of what we are told it was on page eighty-eight; Nigeria, in the author's opinion, was taken over for economic reasons; colonial officials had bad manners (has Thornton ever watched a British resident talking with a native ruler in India, Africa, Arabia, or Malaya?); the British in Mesopotamia after 1918 are said to have been confident and free to do as they liked; the policy aims ascribed to Gertrude Bell were actually more characteristic of A. T. Wilson, whom she successfully opposed.

The author seems to disapprove of imperial government as such. Fair enough. But should we not be shown that other kinds of government in the same time period were better? Is imperialism a distinct genre? Much of the book appears to assume that it is, though in speaking of the arbitrary nature of imperial power, it does not explain in what way and to what extent other types are less so. Toward the end, the treatment becomes so wide ranging that imperialism blends into world politics at large.

The difficulty is familiar. Theorists of imperialism rightly try for an all-encompassing structure. Since the terrain is so vast, the cultures so diverse, the systems so different, and the time span so long, the hypotheses must be stratospheric to meet every

case. As he comes down closer to reality, the theorist must exemplify. Yet the more he does so, the more his generalizations collapse around him. This is a discouraging business, enough so to persuade some that it would be better to look closely at particular cases in great quantity and then see if, in the aggregate, they allowed integrated, summary description. Done properly, this would involve in each separate instance a detailed study of thought and action all along the line from the metropole to the colonial headquarters and on to the local level. If no valid theory of imperialism emerges from this exhausting exercise, might it not yield a helpful assessment of interracial life in a shrinking world?

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STANLEY L. JAKI. *The Road of Science and the Ways to God*. (The Gifford Lectures, 1974-75 and 1975-76.) Chicago: University of Chicago Press. 1978. Pp. vii, 478. \$21.00.

The twenty chapters that comprise this work were originally the Gifford Lectures for 1974-75 and 1975-76. Ranging in time through "twice twenty centuries," and in space from Jean-Sylvain Bailly's strictly conjectural Mongolia (his postulated post-diluvian birth-place of science) to Einstein's Princeton, these essays are works of truly imposing scholarship, delightfully relieved by a wry humor and leavened by a simple and straightforward style.

Stanley L. Jaki's argument runs as follows: (1) belief in the independent existence of a world external to the perceiving subject is the precondition for all science; (2) all science is basically cosmology; (3) cosmology, which is consistent discourse about the universe or "the totality of consistently interacting material things" (p. 262), is, if not convertible to, then at least the basis of, natural theology; (4) natural theology, or "rational concern about the ultimate" (p. 202), demands the metaphysics of moderate realism and an epistemology not befuddled by idealism. (Jaki notes, with ironic approbation, the unwitting truth in Paul Deussen's naive comment that, if children could talk before the age of two, "they probably would talk Kantian philosophy" [p. 127].)

Science, the argument proceeds, requires "unconditional commitment, a most unreasonable posture unless one assumes that the world of facts is always reasonable, which in turn implies that all facts are bound together through an unfailing consistency. Commitment, nay surrender, to that consistency" is, as it was with Einstein, the ethos of science. Character, not intellect, makes the great

scientist. (p. 309). There is but one road-way to science and to God, Jaki argues, and Planck and Einstein are its twentieth-century prophets—often despite themselves, it appears.

The conclusion that Jaki draws from his argument is that Judeo-Christian theism is the historically necessary matrix for the viability of science. The birth of science was neither a logical nor a chronological necessity but a unique historical contingency. Science was born only when men's minds had been "made receptive to natural theology and to the epistemology implied in it" (p. 160) by familiarity with the absolutely transcendent and absolutely rational Creator-God of Christian faith. The belief in a personal and rational creator "cultivated especially within a Christian matrix . . . supported the view for which the world was an objective and orderly entity investigable by the mind because the mind too was an orderly and objective product of the same rational, that is, perfectly consistent, Creator" (p. 242).

Despite the "philosophical" approach taken thus far in the review, it should be stated most emphatically that Jaki's book is not a "metaphysical" account in the pejorative sense of that adjective. His work is very much a work in the history of science. Its claim is that "the road of science, both historically and philosophically, is a logical access to the ways of God" (p. 4). The logic of the history of science—a uniquely contingent development in a uniquely contingent world—is one that can not be determined *a priori* but only *a posteriori*, through empirical, "historical" investigation. And it is as history of science in its more reflective moments that Jaki's work is most impressive. Rarely, if ever, could one discover a finer mastery of sources or a surer discrimination in their applicability. It is not the novelty of this work's argument that makes it so remarkable; it is its cogent and masterful presentation.

Ranging from a deep concern with twentieth-century physics to a broad familiarity with natural theology, Jaki's work is not written for the specialist but for the liberally educated man and woman. If it finds few readers, that would be tragic. But it would simply be a testimony to the tragic state of liberal arts education today.

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OWEN CHADWICK. *Catholicism and History: The Opening of the Vatican Archives*. (The Herbert Hensley Henson Lectures in the University of Oxford, 1976.) New York: Cambridge University Press, 1978. Pp. vi, 174. \$13.95.

One of the great moments in the history of history was Pope Leo XIII's decision, made public in

1883, to open the Vatican "Secret" Archives to all scholars regardless of religious persuasion. Owen Chadwick gives a regrettably brief but fascinating account of the development of these archives, the political and religious upheavals that affected them, and the controversies that finally led to the long-awaited opening. The resulting monograph is tightly organized, reasonably well written, and well researched, making effective use of both published sources and the archives of Pope Pius IX, recently opened to scholars. Best of all, it is written in the same spirit of disinterested scholarship that characterized the author's earlier works on the Victorian Church and the secularization of nineteenth-century Europe.

Catholicism and History is not for the beginning student, for it assumes considerable knowledge of Church history and historiography. On the other hand, it will be useful to more advanced students of nineteenth-century Catholic Church history, giving as it does a more sophisticated insight into the attitude of the Church leadership towards historical scholarship than is found in standard texts. It is clear from this study that the policy of the Vatican toward the use of its archives could not be separated from its approach to the modern world in general. Prior to the elevation of Pope Leo XIII in 1878, many Church leaders, within the Vatican and without, viewed the Church as a beleaguered fortress at war with the modern world. The archives were to help the Church defend itself and carry out its global mission, not to aid scholarship. Carefully selected believers or certain favored governments (including non-Catholic ones like England) might be permitted to see copies of certain specific documents, but unrestricted access to or publication of archival materials was out of the question. Church leaders simply did not believe that non-Catholic scholars could use these materials in an impartial, scientific manner in spite of the pleas of Catholic historians such as Theiner, Acton, and Denifle to give the historians a chance.

Leo XIII had little more faith in fallible humankind and was not basically interested in advancing the cause of historical learning. He was convinced, however, that the interests of the Church could be better served by improving its relations with the modern political, social, and cultural world. To give bona fide scholars unrestricted access to the archives, to tell the full truth about even the most embarrassing chapters in the history of the Church (such as the trial of Galileo) might, in the long run, improve the reputation of the Church among the educated elite; at the very least, the documents could be used to refute the most flagrantly inaccurate mythology about the Church's past transgressions.

Few would now question the wisdom of this policy either from the perspective of the Church's

mission or the cause of historical knowledge. But what, in fact, have the Vatican Archives contributed to either? Here Chadwick's book disappoints, for he deals with this question primarily in terms of what the archives did not contribute rather than what they did. We are left with a few tantalizing hints about the major historiographical revisions that have resulted from Leo XIII's farsighted policy. Perhaps the author will write a sequel that will give us more.

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GUERRINO PELLICIA and GIANCARLO ROCCA, editors. *Dizionario degli istituti di perfezione*. In four volumes. Rome: Edizioni Paoline. 1974; 1975; 1976; 1977. Pp. xxxiv, cols. 1,728; xxvi, cols. 1,726; xxvi, cols. 1,734; xxvi, cols. 1,734. L. 38,000 each.

This major reference work offers a descriptive and historical catalogue of every religious order, whether extant or extinct, in the history of the Christian Church. It is a Catholic enterprise, published in Italian and bearing the clear stamps of ecumenism and *aggiornamento*, intellectual as well as ecclesiastical. First projected in 1950 and worked on steadily since 1956, the dictionary first began to appear in 1974. It thus had a twenty-five-year incubation period marked by momentous happenings, not the least of which was the Second Vatican Council.

The title phrase "institutes of perfection" calls for demystification since this century has made us wary of perfection in whatever context. Modern thought about religious history is, indeed, sometimes beclouded by such wariness. How can it be otherwise if we use archaic language to express, for example, what is probably the most influential sentence in the history of monasticism: "If thou wouldst be perfect, go . . ." (Matthew 19:21). When Malcolm Muggeridge reviewed a book by John Passmore entitled *The Perfectibility of Man* (1970), he delighted in poking fun at the very notion of perfectibility, "the great will-o'-the-wisp of human life, productive of some of mankind's more outstanding achievements, as well as some of its most outrageous follies. No one, I should have thought, looking honestly into his own heart, could suppose himself capable of attaining perfection in any field or in any respect" (*The Observer*, Dec. 20, 1970, p. 21). Muggeridge enjoyed both his fun and his moral superiority without inquiring into the significance of the concept of "perfect." The editors of the *New English Bible*, on the other hand, could not fail to make such an inquiry, with the result that their version of Matthew 19:21 is rather

more instructive, as well as accurate: "If you wish to go the whole way, go . . ." Jesus' teaching is thus presented not as a formula for attaining the unattainable but as advice on how to become a full-fledged (and full-time) member of his religious movement.

We do well to have this version in mind when approaching the *Dictionary of the Institutes of Perfection*. Its preserve includes all officially recognized groups (institutes) of persons who have made complete, full-time vocational commitments to Catholic Christianity. The editors of this project, without having access to the archives of the Sacred Congregation for Religious Orders, set out in search of approximately four thousand institutes; so far they have located five thousand. The dictionary includes articles on the founders and reformers of orders as well as on the history, theology, internal structure, dress, and artistic styles of the orders themselves. The editorial principles, as revised after a general reappraisal of the whole project beginning in 1968 (*annus mirabilis* if ever there was one), call for full attention to recent historiography, to the findings of sociology and psychology, and to the "monastic phenomenon" in non-Catholic Christian confessions and in non-Christian religions. Thus we find articles on Anglicans, Buddhists, Cathars, Druids, and Esenes, to name just a few from outside the fold.

The least engaging articles as a group, but not without interest for modern church historians, are the paragraph-long entries on the hundreds of small, modern orders. Each of these was written either by a member of the order in question or by the editor on the basis of answers to a questionnaire (when founded, by whom, how many houses and members at present, main activities, and so on). There are 150 articles alone beginning with *ancelle* (female servants) and over twice as many beginning with *figli* (sons) or *figlie* (daughters). There are occasional lapses with the major orders, too, as in the case of the anonymous Carthusian who submitted a bizarre, mystical apology for his chosen way of life in place of a history of his order; the editor retained the article but discreetly published alongside it a second one giving what he had asked for in the first place. This second article, by J. Dubois, lays to rest the smug myth, propagated through the centuries by the Carthusians, that they have never needed a reform. But Dubois errs badly in his discussion of Carthusian *conversi* when he says that because of the need for instruction in order to participate in the divine office, "monks cannot but be clerics." This is pure nonsense, easily refuted by centuries of history, to mention only the career and monastic program of St. Benedict. The problem of explaining the role of the *conversi* leads to another weak point when L. Lekai asserts that the Cistercian *conversi* were exonerated

from the duties of the choir, of reading, and of meditation. Was it not instead the presence and labor of the *conversi* that exonerated the monks from the labor required of them by the rule they professed? What would one make now of the claim that noble professors spare university secretaries, librarians, and custodians from the burdens of teaching, studying, writing, gaining recognition, and wearing fancy gowns at commencement?

Of particular note are the instructive articles of J. Gribomont on Anthony the Hermit, of J. Leclercq on the church and monasticism, of E. Werner on the monastic economy (too ostentatiously labeled "A Marxist View of"), of G. Couilleau on the Essenes, and of L. Di Fonzo on St. Francis and the Franciscans. Useful charts, lists, and maps show the chronological and geographical diffusion of the major orders. Monastic architecture gets wide coverage in several well-written and extensively illustrated articles.

The material production of these books achieves a standard worthy of the painstaking editorial preparation. The project first called for three volumes, then six, while at present four have appeared and four more are planned. For once we can applaud inflation. The final volume, whatever its number, is to contain a general bibliography (in addition to those that accompany most articles) and the all-important indexes, which ought to cancel out the inevitable problems that arise from having to reduce such diverse, multilingual material to a single alphabetical order in one language. While the editor continues his great task, may reference departments subscribe and readers begin to explore.

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ROBERT FISHMAN. *Urban Utopias in the Twentieth Century: Ebenezer Howard, Frank Lloyd Wright, and Le Corbusier*. New York: Basic Books. 1977. Pp. xiv, 332. \$13.95.

Robert Fishman's account of the three utopian planners Ebenezer Howard, Le Corbusier, and Frank Lloyd Wright suggests that modern metropolitan regions could be better than they are. Before it was a century old, the industrial city, together with its suburbs and outlying fields and forests, had been irrevocably altered by the growth and dispersal of populations. Since 1950 people have distributed themselves in separate houses scattered over vast areas of land. This dispersal occurred with the aid of the products of modern technology (automobile, electricity, oil furnace, and telephone), of banks and governments (mortgages, loans, highways, zoning, and schools), and of new kinds of production, market, and work (the

large, remote, specialized factory and farm, the warehouse, the truck, the local distributor, and the sales and service industries). Although utopian planners anticipated only a few of those instruments of dispersal, they proposed means of land conservation and ownership, based upon theories of privacy and community, that could have organized such instruments, whether anticipated or not, to form planned urban and suburban landscapes very different from those we know in the megalopolitan corridors and clusters of the United States today.

Essentially three biographies, *Urban Utopias in the Twentieth Century* describes three utopian planners, their ideas, their social innovations, their frustrations, and their often heroic attempts at winning clients to their ideas. The text reveals genuine enthusiasm for its subjects and, by intimate and frequent reference to their writings, conveys their passion and style in contests over theories of distribution of wealth and power in different national settings.

Always scholarly, Fishman documents the development of Howard's Letchworth and Welwyn, Wright's Broadacre City, and Le Corbusier's Contemporary City and Ville Radieuse. He does so by tracing the intellectual, political, and emotional currents of the planners. He treats each planner and each city sympathetically, even in the concluding essay, where he cites Jane Jacobs to question whether any plan or planner is equal to the native energies of people who, left to themselves, will develop, preserve, restore, and recycle buildings, if only local initiative can survive the planners.

This warm, detailed, and sensitive account of three planners, with its good writing and passionate interest in utopian alternatives, has one special virtue and two flaws. Paramount among its virtues is a zeal for urban possibilities. Every student should read this book as an introduction to urbanization and its alternatives, a subject sadly neglected at most universities.

Its two chief defects could not have been overcome except by two different approaches to the topic, and no one has yet mastered both. Both flaws result from the biographical and essentially literary approach. As a consequence, the visual material—plans and drawings—are tertiary documents. Ebenezer Howard was a simpleton designer and as Fishman says, parenthetically, fortunate to attract Raymond Unwin; Le Corbusier was a wizard draftsman and need not have read Saint Simon (Fishman seems to wish he had); and Frank Lloyd Wright's drawings, better than his buildings, make you forget Ralph Borsodi and Silvio Gesell. Planners' drawings, not their texts, are what counts.

It is not really intellectual history either, in the

sense of Lovejoy or Panofsky. Biography dominates over the debates. Take land, for example. Land is fascinating. Its value depends upon what a society can do with it. The period 1880–1930 saw enormous land speculation. After 1886, steel and the elevator rapidly made air rights over six stories valuable. Subterranean rights were valuable for tunnels, subways, railroads, and concourses. A new gateway—a railway or garage or hotel—shot land values upward. All the heroes of this book saw that happening; all their mentors entertained theories about it—Henry George said society at large owned the unearned increment, not the landlord. Thus, the land itself might have been the hero. The right to use land as any owner sees fit is the one right society is clearly abridging—since 1916 in New York—not because of Wright, Le Corbusier, or Ebenezer Howard but because of the abuses they witnessed and protested.

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MARK POSTER. *Critical Theory of the Family*. (A Continuum Book.) New York: Seabury Press. 1978. Pp. xx, 233. \$14.95.

Mark Poster argues that “the major questions of the family concern the psychological level, the types of emotional structure which change as the family changes, generating changes in the deepest needs of individuals” (pp. xviii–xix). It follows that “only psychological theory can enable the investigator to explore the rich set of meanings surrounding feelings between parents and children” (p. xii). But which theory? Those of Freud, Reich, Marcuse, Erikson, Parsons, Lacan, Lévi-Strauss, Bateson, Laing, and many others are all tried by the author and found wanting in various ways. To Poster, Freud is the chief culprit for stressing the intrapsychic development of the individual over the interactional relationships between parents and children and between the family and society and for defending the bourgeois nuclear family with its unique oedipal tensions as the universal norm.

Poster’s own critical theory tries to correct these shortcomings by suggesting that the family is “the place where psychic structure is internalized” by children “and becomes a mechanism for instituting hierarchies of age and sex” (p. 144). In this theory the parental role is paramount, for it is the parents’ response to the child’s oral, anal, and genital behavior, along with the patterns of love and authority presented by the parents, that actually form the child’s sexual and social identity. Because Poster believes that the psyche of the child is no more than its internalization of the ways its

parents actually treat it, in order to understand that psyche we must discover the child-rearing methods used by parents in different periods and cultures. The author reviews four models of the family based on recent work on family and sexual history which show that parents from various social classes responded differently to the oral, anal, and genital behavior of their children in order to create in them a psychic structure synchronic with the parents’ own class interests. Poster emphasizes that he is unable to explain how families change over time and that he seeks only to discover how parents synchronize their childrens’ psychic structure with their own.

Poster concludes with an attack on today’s nuclear family, which he regards as an isolated cell whose members burden one another with romantic demands, where intimate relations between parents and children and concern about the latter’s future have intensified, and where children internalize oppressive and selfish sexual and social identities. Poster would democratize the family by ridding marital and parent-child relationships of their “possessive and devouring” character, by abolishing the ideologies of romantic love and maternal affection, by raising children away from their parents as in the kibbutz, and by designing “specific practices” to eliminate oedipal feelings.

In attempting to avoid the logical circularity and psychological reductionism he mistakenly attributes to Freud, Poster falls into a sociological reductionism with pessimistic implications exceeding those sometimes associated with Freud. Freud, at least, argued that tension always exists between the drives of the individual and the demands of culture, leading to conflict and the possibility of personal and social change. In Poster’s theory children become what their parents make them, and parents themselves are “unconscious agents” of society at large. Poster seems to sense a problem here, for he suggests that “family members must be studied as subjects who internalize structures, but not necessarily in a passive way” (p. 165). But if the content of the child’s psychic structure is only the internalized sum total of sociologically determined parental responses to its behavior, how can the child become anything other than a carbon copy of its parents? Poster can only lamely suggest that “when there is domination there is also resistance,” an optimistic axiom on which many a reformer’s hopes have foundered. By reducing psychic development to the ways parents actually raise their children, Poster undermines his emphasis on the primacy of psychology for family history, in turn reducing that psychology to the trivia of feeding practices, toilet training, and parental reactions to infantile sexuality. Finally, that Poster believes that American children are still raised in

romantic isolation from the hegemonic ideologies and institutions of mass society, or that a further disintegration of the nuclear family will usher in an era of social and sexual democracy must remain a mystery to those who do not share his reformist optimism.

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ALLEN GUTTMANN. *From Ritual to Record: The Nature of Modern Sports*. New York: Columbia University Press. 1978. Pp. viii, 198. \$12.95.

Allen Guttman joins the growing list of scholars who have turned to the study of sport because of its unique relationship with society. As Guttman correctly observes, however, "The question is, what kind of relationship?" (p. 69). The author sets out "to offer a systematic and original interpretation of modern sports and a series of speculations about what is and what is not unique about American sports" (p. vii). He breaks the task down into specific attempts "to define the relationships that obtain among play, games, contests, and sports; to demonstrate what differentiates modern from primitive, ancient, and medieval sports; to interpret the social conditions that led to the rise of modern sports; to comment upon the distinctively American games of baseball and football, and, finally, to look into the American preference for team rather than for individual sports" (pp. vii-viii). Each objective is then explored in its own chapter. Unfortunately, we find more speculation than systematic and original interpretation. Only the objective pertaining to interpreting the social conditions that led to the rise of modern sports lends itself to good cause and effect history. The others lead only to description.

The first chapter presents an adequate summary of previous scholars' research. With the exception of a definition of sport on page seven, however, the chapter offers very little aid for understanding the remainder of the book; Guttman himself admits that he "offered in sketchy and abstract form a preliminary paradigm of play, games, contests, and sports . . ." (p. 13). The heart of the book appears in chapter two, "From Ritual to Record." Here, Guttman lists and analyzes what he believes are the seven distinguishing characteristics of modern sports: secularism, equality of opportunity to compete and equality in the conditions of competition, specialization of roles, rationalization, bureaucratic organization, quantification, and the quest for records. Chapter three, "Capitalism, Protestantism, and Modern Sport," is also important and valuable, because Guttman illus-

trates the advantages of looking at sport from both Marxist and Weberian perspectives and confesses that the preceding seven differences between primitive and modern sports were spurred by "a Weberian view of social organization" (p. 80). There is great potential for this type of theoretical approach, but it was disappointing to see only ten more pages devoted to "A Weberian Interpretation."

Many historians will not be satisfied with the remaining chapters. Readers of *Prospects* will be surprised to find that they have already read chapter four on the "national game," and those relying more and more on quantitative analysis will discover much to be desired in the tables in chapter six. But the major flaw in the final portion of the book is a problem Guttman alluded to earlier when he criticized Marxist and neo-Marxist interpretations as being "the product of ideology rather than the result of careful empirical analysis" (p. 71). The reader is constantly being bombarded with references to fictitious literary characters such as Huckleberry Finn, Henry Wiggen, J. Henry Waugh, and Gary Harkness, and a subsection of chapter six is even devoted to "Fictive Runners." Although the author is at ease when analyzing American literature as it relates to sport—and his ability to handle it is superb—the explanatory power of novels is questionable.

Overall, Guttman achieved what he claimed he would do. Presumably the book will appeal to the "ordinary reader" (p. vii), whoever he or she may be, but Guttman's scholastic peers might expect more—that is, an interpretation of sport that is, in Weberian terms, adequate on the level of meaning and on the level of causation. As it is, the work is too descriptive, too speculative ("may be" and "may have" abound), and more a summary of existing knowledge than a pioneering effort. Guttman does, however, contribute some new material previously untranslated from the German and French, but generally, knowledgeable sport studies scholars will see the same references they have become accustomed to.

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ROBERT G. SUTTER. *China-Watch: Toward Sino-American Reconciliation*. Foreword by ALLEN S. WHITING. Baltimore: Johns Hopkins University Press. 1978. Pp. xiii, 155. \$10.95.

This is a thin book—and it might well have been thinner. Nonetheless, Robert G. Sutter offers much of value. He demonstrates how a CIA analyst, which he was, scours Chinese media for oblique signals of new policy directions or of differ-

ences of opinion among Chinese leaders. His sources are readily available to scholars. He also reminds us how little CIA or State Department officers know of the history of American relations with China—despite a reasonable bibliography.

Sutter's theme is that the Chinese Communists have turned to the United States when difficulties, first with the Kuomintang and then with the Russians, prevented them from attaining their objectives. He argues persuasively that during World War II, again in the 1950s, and in 1969, Chinese overtures were rejected or ignored by the American government. On each occasion, Chou En-lai and other Chinese leaders appeared willing to put aside the dictates of ideology and past differences with the United States to pursue specific ends.

The discussion of the 1940s is probably the weakest section of the book, a troublesome fact, because of occasional comparison with that decade in Sutter's analysis of later periods. The author has not digested the secondary literature and did not use the *Foreign Relations* series for the years after 1945. He exaggerates the seriousness with which those who determined policy responded to John P. Davies's 1944 suggestion of befriending the Chinese Communists to contain the Soviet Union. In 1944 Roosevelt dominated the policy process and he never shared Davies's Russophobia; nor did Patrick Hurley, dominant for much of 1945, show much interest in befriending Mao and Chou. Sutter's claim that, when Acheson sought accommodation in 1949, there was "no corresponding concern in the People's Republic over improving relations with the United States" (p. 32) is embarrassing now that the documents for that year have been published. Mao and Chou certainly were more interested than was Truman.

Sutter does reveal a major Chinese initiative to which the United States was unresponsive between 1955 and 1957. Although Dulles agreed to begin ambassadorial-level talks in Warsaw, he was not interested in rapprochement with China and sought a propaganda victory instead. In 1969 a brief Chinese initiative sputtered, apparently under attack from "radicals" opposed to improving relations with the Americans. Sutter notes that Chou, embarrassed by several failures to achieve anything through friendly gestures toward the United States and struggling to protect the foreign policy apparatus that had been swamped during the Cultural Revolution, responded gingerly to Nixon's subsequent overtures.

Sutter's references to occasions when Peking muted the Taiwan issue promise a solution to the problem of the American protectorate there. If Americans will settle for something less than explicit assurance that the People's Republic will not use force to liberate the island, they may indeed

have their coveted two Chinas. But he also argues that the Chinese are interested in good relations for the purpose of gaining strategic advantage against the Russians. He warns that if the United States reduces its forces in East Asia or fails to demonstrate the will to contest the expansion of Soviet influence, the Chinese may lose interest and seek reconciliation with the Soviet Union. His message, that considerations of power politics and not mutual affection bind the two nations for the moment, is salutary. In part, Sutter's concern for maintaining the Chinese connection echoes the views of opponents of détente with the Soviet Union—and this is a useful reminder that the new relationship with China may not be without cost.

WARREN I. COHEN

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ANCIENT

N. K. SANDARS. *The Sea Peoples: Warriors of the Ancient Mediterranean, 1250–1150 B.C.* New York: Thames and Hudson. 1978. Pp. 224. \$12.95.

The "Sea Peoples" is not their own name, but an Egyptian metaphorical description, perhaps even mistranslated; it is clear that to the ancient historian it means a complex, fascinating puzzle. The central problem is whether the widespread catastrophes (for example, the destruction of Mycenaean cities in the thirteenth and twelfth centuries B.C. and the fall of the Hittite empire) at the end of the Bronze Age in the Aegean and the Levant, for which there is only archeological evidence, can be attributed to the disruptive movements of those who appear in Egyptian records in alliance with the "Sea Peoples" as attackers of the possessions of Egypt. If the same peoples could be traced in the archeological destructions, contemporary Egyptian, Ugaritic, and Hittite documents might provide a framework, if not an explanation, for the ending of the Bronze Age. How that is to be done and whether it may properly be done at all are more critical problems. The great merit of N. K. Sandars's book is that the intricacy of the problems, not her solution, is given emphasis.

There is a good account of the geographical and climatic background of the region indicating the fragility of the support it gives for the survival of advanced civilizations. The archeology of the Balkans, the Aegean, Asia Minor, and much of the Mediterranean occupies two chapters. The variety of the materials and the range of sites are represented by a good selection of illustrations. The means for tracing cultural contacts and synchronisms are illustrated chiefly by weapons and a little pottery. The narrative of these chapters is

best appreciated in the chronological tables newly devised and ingeniously arranged for this volume, but only the reader already acquainted with the sites will be at home here.

In the remaining chapters Sandars shows that many ancient texts will yield reliable economic information and add directly to the archeological picture but that the tempting documents for events are scattered, diverse, and harder to control. To make a history of this period one must place on the map the recorded and pictured attackers of Egypt, from the Battle of Kadesh to the Libyan Wars of Rameses III, trace their movements, and, as far as possible, identify them with historical peoples known from Ugaritic, Hittite, or other documents. Sandars takes pains to show how difficult this is, how uncertain, for example, are the equations of names in Egyptian language and orthography with names in other scripts and languages, and in other centuries, too. Yet she shows that an intelligible account can be drawn even from this evidence.

It is no wonder that many are interested in the "Sea Peoples." This book will certainly stand as a good introduction, but other studies will continue to appear. The large number of references in notes to other scholars with work in progress will indicate not only how thoroughly Sandars is up to date on current research but also how vigorous the field is.

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DAVID STRONACH. *Pasargadae: A Report on the Excavations Conducted by the British Institute of Persian Studies from 1961 to 1963*. Oxford: Clarendon Press of Oxford University Press. 1978. Pp. xii, 326. \$62.50.

For all his well-known accomplishments, Cyrus the Great (559–530 B.C.) remains a shadowy figure. Thus Pasargadae, the capital built by Cyrus in the Fars region of Iran, has attracted the attention of archeologists, historians, and art historians in an effort to learn more about Cyrus and the origins of Achaemenian kingship, life, and art, all of which are so brilliantly, if still enigmatically, seen in the slightly later site of Persepolis. Now David Stronach presents the results of his excavations of 1961–63, along with a synthesis of the other work done on the site, in a brilliant study with carefully drawn plans, good plates, and well-reasoned arguments.

After giving the history of the discovery and excavation of the site, Stronach carefully describes and discusses the date and function of each building and the cultural influence it represented. In the

latter area, in particular, he relies heavily on the work of Carl Nylander, *The Ionians at Pasargadae* (1970); but at many points Stronach is able to correct and amplify Nylander on the basis of new information. Under Stronach's hand Pasargadae is seen for the major construction and monument that Cyrus intended it to be. Here as in other areas Cyrus blended contemporary Ionian, Lydian, Babylonian, and Iranian elements into a unified, expansive whole.

Stronach attributes the first phase of the Tall-i Takht, the Tomb of Cyrus, Gate R, Palace S, the Zendan, and some of the smaller constructions to Cyrus. He refutes G. M. A. Richter and Henri Frankfort's position on Greek workmanship and agrees with Nylander that, although Greek influence is present, "the Pasargadae artists were 'orientals trained in oriental traditions'" (p. 77).

For Palace P, Stronach's plan differs from and clearly supersedes all previous efforts. Although the building is one of the last of Cyrus's efforts, the relief sculpture must be placed ca. 510, under the reign of Darius. This "fully confirms Darius not Cyrus as the creator and codifier of classical Achaemenian art" (p. 106).

The structures investigated give some idea of the importance of the site for Cyrus and the later Achaemenians, but Stronach's plans and pictures do little for recreating the glories of the concept. For this, some very small remains are crucial. The excavations have come up with two small pavilions and a number of water channels. In a deceptively simple and understated chapter Stronach, stressing the Persian love of gardens, recreates the Royal Gardens in and around Gate R, Palaces S and P, and a later Achaemenian bridge. These constructions along with the aerial photograph of the site taken in 1933 (Schmidt, *Persepolis*, I [1953], fig. 3) show the presence of ample water for horticulture. While Stronach confines himself to the investigations in the Palace area, the paths that connect these buildings should probably be extended to the Tomb of Cyrus, the Zendan, and other buildings. Further, if these royal paths had also been landscaped, the entire 44 sq. km. park would have made a veritable Versailles! Could the gardens themselves have been the major monument of Pasargadae and the buildings only the ornament?

With the wealth of archeological material present, the historian will be disappointed in the short, rather perfunctory discussion of the "historical considerations." It does little to relate the finds to the historical picture of Cyrus; indeed, throughout the book Stronach seems intent on the surface aspects with only occasional attempts at wider interpretation. This reader missed a more detailed discussion of the important masons' marks which supports the conclusion that many of the masons

worked in Ionia and Lydia. Also a more expanded, illustrated treatment of the Babylonian and Assyrian aspects of the Pasargadae reliefs would have been welcome.

The production of the book is handsome, but there are some signs of haste which will confuse the reader: the key to figure 3 is wrong. An examination of figure 4 (which uses the same key) will allow corrections. Plates 182–85 seem to have been inserted after the text was set, with the result that a number of plate references are incorrect. Further, the drawings in the catalogue of objects are not fully cross-referenced with the plates. Finally, plate 193 is missing.

These last comments are admittedly minuscule blemishes and inconveniences in a major work that will instruct and fascinate the reader interested in any aspect of the Achaemenid world.

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KARL-WILHELM WELWEI. *Unfreie im antiken Kriegsdienst*. Part 2, *Die kleineren und mittleren Griechischen Staaten und die Hellenistischen Reiche*. (Forschungen zur antiken Sklaverei, number 8.) Wiesbaden: Franz Steiner. 1977. Pp. 185. DM 54.

In this work Karl-Wilhelm Welwei continues the survey that he began in volume one with an examination of the “unfree” (there is no good, equivalent term in English) in the military service of Athens and Sparta. In dealing with the remainder of the Greek world in this present volume, he abandons any geographical categorization and arranges the material according to the circumstances or uses of the slaves/serfs in the military sphere. Three broad divisions are presented: service in civil wars and internal struggles, service in the fleet, and service in the land forces. This last category is further divided into five areas: mobilization of “unfree landholders” and the use of slaves/serfs by tyrants, by besieged cities, in uprisings against Rome, and in noncombatant roles. Within each of these categories Welwei approaches the material in a generally chronological fashion.

The chief value of this book is that it collects and arranges a great deal of diverse material pertaining to a single subject. In fact, the introduction to the first volume implies that this was one of Welwei’s goals, and he seems to have fulfilled this difficult task. He has examined, so it seems, every important instance of the military use of servile groups. The work is a useful reference for those interested in any aspect of the involvement of the servile classes in ancient warfare.

It is hard to find any serious fault with this book. A common shortcoming of works of this type is

that the great number and variety of instances with which the author must deal precludes exhaustive consideration of every example. The reader must guard, therefore, against being too quickly convinced by this or that single case. This hardly detracts from the worth of Welwei’s book, however. Judging from those cases that I am best qualified to evaluate—those dealing with the Rhodians—I conclude that he has been thorough and fair in his scholarship.

Welwei’s apparent reluctance to generalize and draw conclusions from his material is more a disappointment than a fault. Caution ought not to be thrown to the winds of course, but Welwei provides no conclusions in the sections dealing with “unfree landholders,” fleet service, and support of tyrants; and generalizations are sparse in some of the others. I would have liked to have seen more of the kind of comments that are made in the exceedingly brief concluding chapter (6 pages out of the 355 of the two volumes).

And finally, why in a book dealing with the use of slaves/serfs in warfare is there never any real discussion of just what methods and problems were connected with the actual process of turning a slave into an effective soldier, especially of the more heavy-armed type?

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JOHN LEACH. *Pompey the Great*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 265. \$13.50.

Political biography as a genre has recently run into heavy weather. Critics condemn it for narrowness, for excessive concern with personality, and for a tendency to isolate the individual from the social and cultural fabric of his time. Yet biographies still roll off the presses. Military and political figures of the late Roman Republic are especially in vogue. Shelves bulge with studies of Julius Caesar. The Cicero industry seems to produce a volume almost annually. The last year has witnessed the appearance of two biographies of M. Crassus, and one on M. Antony, and there is still another in press. John Leach took an especially worthy subject, Pompey the Great, perhaps the central figure of the Republic’s last generation. Until now, surprisingly, he has not received a full-scale treatment in English. But Leach will not long have the field to himself. A scholarly biography of Pompey by Robin Seager is now pending. The industry’s wheels continue to turn.

Leach’s study has considerable merit; a moderate and balanced account, developing Pompey’s career in the context of the political and social scene of the late Republic, it is an objective treat-

ment, sound and reliable, with no special axes to grind. Leach competently outlines Pompey's rise to authority, the sources of his power, his relationship with rivals and subordinates, his military campaigns, his behavior abroad, and the final break with Caesar that led to disaster. Though not marked by verve and vivacity, the book is eminently readable.

Pompey has been an enigma to contemporaries and scholars alike. His ruthlessness in civil war contrasted with his generosity toward foreign foes. He had a passion for special privileges, but leaned to "constitutionalism." He engaged in political strife, but desired to stand above it. His cultivation of the nobility contrasted with his relations to its critics. He was occasionally forthright but customarily ambiguous. Leach does not solve or even confront all the riddles. The weakness in this sort of biography is not narrowness of orientation but adherence to a rigid chronological narrative that prevents the author from pulling threads together. Several lines of inquiry that could illuminate Pompey's aspirations and personality receive scattered mention on occasions when they come into the story but are never really developed. Some of the more promising and untied loose ends include: Pompey's supposed connection with a "reform movement" in the 70s and 60s (pp. 56, 62, 64-65, 103, 106-07, 109, 122), curiously forgotten when Leach comes to his documented reforms of 55 and 52 (pp. 149, 157-60); the intellectual interests of and their influence upon Pompey (pp. 22-23, 72, 101, 133-34); the emulation of Alexander (pp. 31-32, 53, 78); the financial obligations imposed by Pompey on foreign potentates (pp. 89-90, 124); and his attitudes toward the governance of overseas provinces (pp. 64-65, 73-74, 78-79, 88-89, 98-100, 122). These and other matters would have benefited from an integrated treatment instead of isolated allusions. The result is a fragmented portrait of Pompey: "a highly complex character, hard to understand, sometimes infuriating, often admirable" (p. 213).

Notwithstanding this fault and the fact that Leach's study has little new to offer for scholars, it will serve as a lucid and valuable introduction to the subject for undergraduates.

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JOHN NICOLS. *Vespasian and the Partes Flavianae*. (Historia, number 28.) Wiesbaden: Franz Steiner. 1978. Pp. 186. DM 38.

This is a good book, but it could, and should, have been much better. John Nicols controls a vast

amount of material with admirable caution and considerable sensitivity, but seldom have I been subjected to so much carelessness, to such inadequate proofreading, to such strange syntax, and to such remarkably idiosyncratic punctuation as this rather slender monograph inflicts. From the very first footnote the reader must be alert to error. It is a pity that the *summa manus* is so dreadfully lacking, for the author has something to say.

He has undertaken to trace the support for Vespasian that made it possible for him to lay claim to the purple with very good prospects of success. The *partes Flavianae* embraced many men the empire over, who favored him with varying degrees of enthusiasm. Some, such as Tiberius Julius Alexander, the prefect of Egypt who forced the issue by having his troops hail Vespasian as emperor before the latter's own soldiers did, and Antonius Primus, who acted on his own in causing some of the Danubian legions to invade Italy in advance of Mucianus, were instrumental in forcing events; others were more cautious and put their names and influence at Vespasian's disposal only when the outcome became clear. Nicols is quite right in underscoring the great differences in support for Vespasian in different places. He undertakes to establish the identity of "Vespasian's party" by linking individuals and the progress of their careers with the Flavian family. This, the prosopographical approach, is frequently the only tool the historian possesses in attempting to determine the course of history, but it seldom can offer a sure answer, although it can suggest what might have happened and who was involved. There have been recent objections to the prosopographical method, but Nicols usually refrains from pushing likelihood too far.

Much the best part of the book is part two, wherein he undertakes to sort out the confusing chronology of events from 67 to 70. His argument is lucid and convincing. For the remainder, when dealing directly with Vespasian's supporters, there is less that is novel. The great value of these sections is the ordering of men and events, so that the reader can gaze upon the world-wide stage with a wider vision.

Nicols gives the impression that Vespasian's ultimate choice as a candidate for the empire was inevitable and that, of the two great governors in the east, Mucianus and himself, he was clearly the more likely choice. I am not fully convinced. Both had faults, yet Tacitus speaks of Mucianus's *magnae virtutes*, and certainly a Licinius had at least as much family background as a *novus homo*. Vespasian commanded an army in the field, but Mucianus's legions were also well-tested; Vespasian had sons, and herein lies the key, as Mucianus himself said (Tacitus, *Hist.* II 77. 1). But things

could easily enough have gone otherwise, and we might then have before us a volume entitled *Mucianus and the Partes Licinianaë*. History has produced stranger results than that.

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T. D. BARNES. *The Sources of the Historia Augusta*. (Collection Latomus, number 155.) Brussels: Latomus Revue d'Études Latines. 1978. Pp. 135. 450 F.

The *Historia Augusta* (*HA*) is an important source for the history of the Roman Empire, covering the emperors from Hadrian to Carinus, A.D. 117 to 285, but modern historians differ on the reliability of the information it contains. T. D. Barnes presents a brief, forthright argument for the value of both the *HA* as historical evidence and the sources from which it derived its information. Barnes follows Hermann Dessau and his own teacher, Sir Ronald Syme, in arguing that the *HA* had a single author who, Barnes says, wrote between 395 and late 399 and not the six authors the text claims wrote under Diocletian and Constantine. Barnes chooses to devote little space to substantiating his description of the author as a historical romancer who relied heavily upon a series of single sources, to which he added as he pleased from his own imagination or from other accounts.

There is no question about Barnes's positions on the problems he raises, although he admits that his analysis of the sources provides "no final certainty, only the conviction of the proper balance of probabilities" (pp. 18-19). A text of the *HA* must be near at hand as well as texts of many of the ancient sources cited in order to assess Barnes's conclusions, especially in the longest chapter of the book (chap. 4), in which he deduces the quality of the various biographies from the *HA*'s use of specific names, dates, and events. Scholars can find Barnes's conclusions on individual passages easily by using his "Index of Passages."

The more interesting and readable part of the book contains Barnes's description of the sources for the *HA*. Barnes concludes that the *HA* found its authentic information in six main sources: Ignotus, the unknown Latin biographer, was used for the period prior to 217; Marius Maximus served as a supplement to Ignotus and as the main source in *Heliogabalus*; Herodian was the basic source for 238 but was also used earlier; the Athenian Dexippus supplemented Herodian and was the main source from 238 to 270; Enmann's postulated *Kaisergeschichte* was used especially after 260; and the *History* by Eunapius was used for the years after 270. Barnes also documents that the epitomators Aurelius Victor and Eutropius were used

in several places and suggests the possibility of the use of Festus and Ammianus Marcellinus. Perhaps the most interesting argument is a chronological adjustment that places the first edition of Eunapius's *History* not long after 378 instead of shortly after 395. This dating has important consequences for other authors such as Ammianus and the author of the *Epitome de Caesaribus*, and Barnes's conclusions merit further study.

Much of this book has the dryness and physical layout of a line-by-line commentary on the *HA*, and it is relieved only by the sharp words Barnes has for many of his predecessors. In the last chapters of the book, however, Barnes's solution to the vexed problem of the sources of the *HA* is controversial and stimulating. One can quibble with his conclusions on individual passages, but the concise discussion of the interrelationships of the literary sources for the second and third centuries is a valuable contribution.

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Harvard University

MEDIEVAL

ACHATZ VON MÜLLER. *Gloria Bona Fama Bonorum: Studien zur sittlichen Bedeutung des Ruhmes in der frühchristlichen und mittelalterlichen Welt*. (Historische Studien, number 428.) Husum: Matthiesen Verlag. 1977. Pp. 200.

Any reader of Marco Polo must acknowledge that, at least on the level of high culture, European peoples shared negative propositions about fame and glory that excluded a very mixed bag of practices: tattooing the body, sexual liberality, "idolatry," ritual murder and festal cannibalism, using paper money, burning the dead, and daily washing, especially washing before meals. Such acts were considered shameful in Europe; some prohibited on pain of death. Within broad forbidden limits, enormously varied convictions grew up. Did they have a nucleus in common, as well as outer limits? The object of the present book is to blaze a path to the Grail at the center of this luxuriant forest of belief.

The book under review belongs to a school of historiography whose goals are clear, but whose methods and guiding theories are fluid. The school is dedicated to the history of concepts (*Begriffsgeschichte*), and two of its major practitioners, the eminent medievalists L. Buisson and O. Brunner, advised Achatz von Müller as he completed the dissertation out of which *Gloria Bona Fama Bonorum* has emerged. The history of concepts is not quite the same as the history of ideas. The latter tends to

regard ideas as principles, or units, that endure, however differently they may be understood. Although it rejects historical relativism, the former portrays the concept as something built up out of highly diverse components over time, an artifact that will disclose much of value concerning both ageless quandaries of human existence and time-bound social values, if only it is properly broken apart and analyzed. On the whole, the existing methods for the analysis of concepts have been most successful when applied to short-range developments in a single language; for then an investigator can identify and, perhaps, explain the variety of connotations that any given concept may have had for particular authors. The methods have been least satisfactory when applied to many authors, writing in different languages, over long spans of time; for these efforts require such a high degree of schematization to achieve thematic unity and coherence that an author may be forced deliberately to obscure the true intricacies of his evidence. Reviewing in brief the concept of fame from Homer to Dante, the present study, despite considerable achievements, labors under the methodological insufficiency of its genre.

What has the author achieved? By approaching fame as a moral concept, he has been able to identify two definitions that co-existed throughout European history. The broader one construed fame as a kind of secular immortality bestowed by a class, such as Homeric warriors, or by society as a whole. The more restricted one defined it as the imperishable crown conveyed to good men and women through their common participation in good itself. The author holds that the first definition could exist separately, as it did in Homer's mind and in the secularized view, current in late thirteenth-century Florence, that the glory of the city subsumed that of the individual. But, the author holds, both definitions frequently co-existed in a single mind. This was particularly so when a person assumed that he was responsible to two moral orders: the one prescribed by law and custom and a second, higher one written into the nature of things. This was the case with Plato, Aristotle, and Cicero; it figured in Thomas Aquinas's distinction between glory in the natural and in the supernatural order; it achieved a poetic apotheosis in Dante's apparent conviction that glory, as fame in the natural order, gave the damned a legitimate secular compensation for the beatitude that they had lost in the hereafter.

The author's primary concern is with the moral definition of glory. Tracing its history, he considers adjustments required, first, by the introduction of the Christian virtues of humility and charity, and, later, by the interchange between Germanic

and Christian ethical systems. Conceiving an enterprise of this scope requires exceptionally wide architectonic vision; throughout, the author has informed his task with massive bibliographical knowledge. And yet, the method employed portrays a steady, unilinear development that does not correspond with the messy facts. The author's need for narrative unity has forced him to select evidence according to principles that are not always clear. The most patent instances of this apparently random selectivity are the discussions of visual arts with which the author wishes to illustrate his conclusions about patristic and early medieval thought. The materials adduced are too few to be representative; no attention is paid to the variety of contemporary styles; and the diversities of provenance between the artistic and documentary evidence are not explained. Moreover, if visual arts do substantiate the author's propositions, why is there no discussion of them with regard to classical Athens and Renaissance Florence, the termini of his account?

If the reader engages in an active dialogue with the author, he is bound to raise similar questions with regard to the selection and treatment of documentary evidence. The selection of "great books" insures that the texts considered stood head and shoulders above what was typical in their respective ages. Had the author of the present study limited himself to identifiable traditions—in literature or theology, for example—he might still have drawn a convincing pattern of development from his evidence. The tradition would have selected his landmarks for him. But, since the author's method has led him to cross the boundaries of cultures and literary genres, his evidence does not represent any more than it typifies. Often one has the feeling that the author could have told an entirely different story by choosing other texts, or even other passages from the same "great books" that he does discuss. For, during its long life, any concept appears, not as a single theme unfolding, but rather, to use Braudel's phrase, as "a series of overlapping histories developing simultaneously."

Despite these shortcomings, the author has found his Grail by identifying the European concept of glory with the metaphysical concept of the good (and, conversely, with the problem of evil). Constantly rebounding between the erudite and the popular levels of culture, this metaphysical and moral vision did profoundly distinguish the Western quests for fame from their oriental counterparts witnessed by Marco Polo. We are obliged to the author for having given it the emphasis of his learning and insight.

K. F. MORRISON
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MAHLON H. SMITH III, *And Taking Bread . . . : Cerularius and the Azyme Controversy of 1054*. (Théologie Historique, number 47.) Paris: Éditions Beauchesne, distributed by Beauchesne-America, South Bend, Ind. 1978. Pp. 188. Cloth \$20.00, paper \$15.00.

In recent years, historians have consistently minimized the significance of the schism of 1054. The date does not mark the definitive rupture between the Latin and Greek Churches. Their division, on the contrary, was a progressive detachment that culminated in the crusade of 1204; it is then that the schism took on both importance and significance. Equally, the explanation for the origins of the ecclesiastical conflict of 1054 has been as consistent as it has been conventional. This explanation, embodied in such classic studies as those of Bréhier, Norden, Will, and, above all, Michel, interprets the conflict as the deliberate product of an ambitious patriarch, Michael Cerularius. This "malicious schemer" is the individual who must be held responsible—so it is argued—for initiating the azyme controversy, the quarrel over the correct recipe for the sacramental bread of the Eucharist. He thus raised an issue that today is almost universally rejected as a cause for separation.

And Taking Bread . . . is a generally successful attempt to counter this interpretation by showing that the azyme controversy was not instigated by Cerularius and that he was not personally responsible for creating the quarrel with the Latin Church. Ultimately, the author believes, the root cause lay in the liturgical traditions of the two churches and especially in their mutually opposed theological explanations of the origin of the Eucharist and of the relationship of Christian faith to Jewish tradition. On the whole, the author's arguments—that the religious turmoil in southern Italy existed even before the earliest documented Byzantine intervention; that Cerularius probably depended for his arguments on Leo of Ochrid; that Cerularius consistently misunderstood Humbert's mission; and that the contents of the Greek documents actually contradict the contention of historians that Cerularius deliberately raised the issue of azyms—are at once compelling and persuasive.

The author's thesis is not entirely new. The belief that Cerularius was not directly responsible for the sensational events of July 1054 has been expressed before, as has the notion of the contradictory liturgical traditions of East and West. It has been noted, for example, that Cerularius's autumn 1053 correspondence with the pope, in which he proposed a resumption of *communio in sacris*, indicates that the banal dispute of rites was never considered by Cerularius as a real obstacle to rela-

tions and unity with Rome. Even so, neither idea has really been explored or documented in any depth. As such, this book, with its nonapologetic, nonpolemical perspective, fills a need and is important. All the same, the study is much too brief; the liturgical traditions of the churches should have been explored in greater detail. Unaccountably, too, the book is thin on bibliography; for example, the key article of J. H. Erikson on the Byzantine understanding of the eucharistic bread is missing.

ARISTEIDES PAPADAKIS
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HANS EBERHARD MAYER. *Bistümer, Klöster und Stifte im Königreich Jerusalem*. (Schriften der Monumenta Germaniae Historica, number 26.) Stuttgart: Anton Hiersemann. 1977. Pp. xxvii. 438. DM 130.

Hans Eberhard Mayer, the distinguished authority on the crusades, offers here an extraordinarily learned treatment of bishoprics, monasteries, and other religious establishments in the Latin Kingdom. He begins with the Church of the Holy Sepulcher and the attempt of the patriarch Daimbert to make the kingdom a fief of the Church. He then unravels the complicated tale of the creation of the bishoprics of Ascalon and Bethlehem, the former founded as a step toward a bishopric of Bethlehem and becoming a priorate after the establishment of the latter. The confused beginnings of the bishopric of Tiberias follow. He moves on to Tyre, where the foundation of the archbishopric is a well-known story, although the author warns us to distinguish between the original cathedral (the *Marienkirche*) and the cathedral of the crusading period. He then returns to Ascalon to recount the second establishment of a bishopric there. In Samaria the abbot of the *Templum Domini* took the place of a bishop. The author discovers a grand design by the king and the patriarch in 1168 to found new bishoprics in the south in support of an attack upon Egypt. The plans were terminated by the rise of Saladin.

Mayer then turns to monasteries. S. Maria Latina was a precrusading Benedictine house. The *Templum Domini*, a house of secular canons, was founded just after the conquest, like the canons of the Holy Sepulcher. The monastery of Mt. Sion was established in the time of Godfrey of Bouillon, though not by him. The latter tradition was based on a forged document. Then he takes up the cloister of St. Anne in Jerusalem. There is a very long chapter, with several subsections, on the Benedictine cloister of S. Maria in the Valley of Josaphat. The double monastery (monks and nuns under an

abbess) at Bethany is also treated. So are a lone Cluniac cloister in Acre and two Greek communities.

The work as a whole is an excellent example of good, old-fashioned, Germanic scholarship. It is full of intricate detail based upon a very minute analysis of primary sources and a familiarity with the literature. The amount of information that Mayer is able to deduce from witness lists, for example, is really remarkable. He dates questionable documents with ingenuity. His skilled detective work proves many documents to be forgeries. Persons and places mentioned in the documents are identified, often with laborious, but convincing argument. Mayer is always judicious and fair.

The detail is, however, overwhelming. When a pond is granted, the author not only identifies it, but gives its length, width, and depth. On the admittedly more important question of whether the cloister of S. Maria in the Valley of Josaphat was founded by Godfrey of Bouillon he spends twenty-nine pages. I am tempted to say that only highly specialized scholars will take advantage of Mayer's great learning, but that would not be quite fair; if I, for example, were to work on the Venetians in the Holy Land, I would have to refer to Mayer's discussion of the documents concerning Venice. (His *Register der Behandelten Urkunden* will be very helpful to various researchers.) Possibly only specialists will want to read the book cover-to-cover, but others will have to refer to it for scraps of information and to avoid relying upon forged documents.

The book ends abruptly without a conclusion. I would have been interested in Mayer's view of the significance of this remarkable display of scholarship.

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JEAN LONGNON. *Les Compagnons de Villehardouin: Recherches sur les croisés de la quatrième croisade*. (Centre de Recherches d'Histoire et de Philologie. Hautes Études Médiévales et Modernes, number 30.) Geneva: Librairie Droz. 1978. Pp. 267.

This work is a biographical register of the participants of the Fourth Crusade indicated by the expedition's chief chronicler and one of its leaders, Geoffrey of Villehardouin, and by other literary sources. The author groups these knightly and clerical dramatis personae according to the eight geographic regions from which they derived.

The high quality of Jean Longnon's study results from the comprehensive data he has collected about his personalities. In addition to accounts of

the crusaders' martial exploits, itineraries, and attitudes toward the diversion of the crusade to Christian Zara and Constantinople, by scrupulous investigation of local Western sources he has added career information relating to the participants' home lordships. By creating this illustrative sample of the *militia Christi* and of the founders of the Latin Empire, Longnon adds further detail to currently known characteristics of this movement, such as the crusaders' close family and feudal and geographic bonds. He also strengthens the argument that the unexpectedly small muster of crusaders at Venice in 1202 created a shortage in the payment for transportation pledged to the maritime republic, a fiscal predicament that allowed the expedition to be diverted from its original destination of Egypt. Of considerable interest is the accumulated evidence that the crusaders' zeal owed much to previous experience or to the inspiration of crusading relatives. From this last point it is apparent that crusading acquired a self-generating momentum in the twelfth century. The sagas of this Western crusading and colonizing aristocracy also offer much to those interested in manifestations of the West's growing expression of individualism.

If there is a reservation about this work's format, it would be the lamentable absence of a topical index to aid in its mining.

ROBERT B. PATTERSON
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GUY FOURQUIN. *The Anatomy of Popular Rebellion in the Middle Ages*. Translated by ANNE CHESTERS. (Europe in the Middle Ages, Selected Studies, number 9.) New York: North-Holland. 1978. Pp. xiv, 181. \$28.25.

By the author of *Histoire économique de l'Occident médiéval*, which appears in the excellent Collection U series of studies on the Middle Ages, this work was published originally in 1972 as *Les soulèvements populaires au moyen âge*. It has been well translated for the new series Europe in the Middle Ages: Selected Studies, edited by Richard Vaughan, which is making available in English some seminal works of Continental scholars published since World War II. Guy Fourquin indicates his purpose clearly when he states, "This book is an attempt to 'unlock' the history of rebellion in the Middle Ages in order to place medieval history more within reach of the modern cultured reader" (p. xiv). To achieve this end he organizes the popular uprisings into three categories: messianic movements (including such disparate groups as participants in popular crusades, adherents of the ideas of Joachim of Fiore, flagellants, and advo-

cates of egalitarian millenarianism), rebellions connected with the problems of social mobility (such as the attempts of guildsmen to accede to the rank of the elite, as in Flanders in the fourteenth century, or movements by one elite against the political elite, as in the case of Etienne Marcel), and rebellions linked to crisis situations (most especially the uprising in Flanders in 1323–28, the Jacquerie of 1358, and the English Peasants' Revolt of 1381). Although the work focuses primarily on the fourteenth and fifteenth centuries and does not contain every popular uprising—it does not include, for example, the Sicilian Vespers or the uprisings of the Slavs against their German masters in the tenth century—it succeeds through the tripartite division in presenting a more coherent picture than would have been possible using a chronological or regional framework.

As useful as the section on the typology of rebellions is, most historians will find more suggestive the initial portion of the book. Here Fourquin examines the sociological methodology he is employing and offers definitions of such key terms as "revolt," "revolution," "elite," and "class." Particularly interesting are his negative comments on the application of the Marxist interpretation of class conflict to the medieval world. More references to contemporary Marxist scholarship would have made his argument even more valuable.

The book has several unfortunate weaknesses. The price of \$28.25 is a substantial sum of money to pay for a work of one hundred and sixty pages having no footnotes and containing only a brief bibliography, that primarily of the studies of French scholars. Many readers will find annoying the lack of identification of sociologists and historians, or their works, in the initial portion.

These deficiencies aside, Fourquin does succeed very well in his purpose. This study is a provocative introduction to the subject and should, with other recent works, especially M. Mollat and P. Wolff's *The Popular Revolutions of the Late Middle Ages* (1972), hasten further research on the topic.

DANIEL CALLAHAN
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R. A. DONKIN. *The Cistercians: Studies in the Geography of Medieval England and Wales*. (Studies and Texts, number 38.) Toronto: Pontifical Institute of Medieval Studies. 1978. Pp. 241. \$18.00.

The Cistercian order is a prime example of the medieval ability to turn ideas into institutional reality. The order was founded at the turn of the twelfth century, part of a general European wave of ascetic enthusiasm and reaction against the world and "soft" monasticism. With a constitu-

tional network linking mother houses to daughters or colonies, and with an immediate appeal to wealthy laymen as well as to would-be-monks and affiliated lay brethren, the order spread rapidly. By 1151 there were over three hundred houses, reaching from Norway to Hungary and Sardinia. The white monks arrived at Waverly in Kent in 1128, and by 1152 there were over sixty houses in the British Isles. Nor was all to be told in endowments and buildings: in St. Bernard the order boasted of and was headed, *de facto*, by the twelfth century's spokesman for rigorous, militant, mystical Christianity.

In the realms of economic and social history the Cistercians are usually given credit for the Protestant virtues. They were hardy pioneers, clearing new land. They were canny shepherds and wool merchants. They eschewed the old habitats of men and sought the wild hills and valleys to raise their huge flocks. They grew rich in the process and built such houses as Fountains, Rievaulx, and Tintern. Before the inevitable decline in zeal and lay support, they were established as wealthy fixtures of the rural landscape.

R. A. Donkin's study is a model essay in historical and economic geography. He forces us to revise the supple generalizations of the textbooks (and our lectures). The Cistercians were not really such pioneers of the virgin wilderness. Rather, they extended the limits of settlement mostly by pushing the frontier along in smallish steps. The amount of land they cleared *de novo*—as assart—was always limited, and they were not particularly innovative. In some places their acquisition of land caused depopulation, for they preferred barren sheep runs to existing villages. In others they helped appreciate land that in the 1140s was still suffering from the Conqueror's attentions. Their relations with patrons were mostly harmonious, and they showed enterprise in consolidating their holdings so as to create the familiar Cistercian grange, the "directly cultivated and relatively consolidated land holding with its own nucleus of farm building." Their wealth, if based more on sheep than on any other single resource, was quite varied in nature. Agriculture, pastoral activity in cattle as well as sheep, timber and forest perquisites, markets and fairs, fulling mills, and even some urban real property all played a role. By the late thirteenth century we are dealing with monastic houses capable of transporting their own wool across England and of dealing directly with such Italian merchant-banking families as the Peruzzi, the Bardi, and the Frescobaldi.

Donkin recounts his material clearly and concisely. His tentative generalizations offer a conservative assessment of Cistercian activity and creativity. His material is supplemented by

twenty-five maps, seven tables, eleven appendixes, an extensive bibliography, and four indexes. As befits a historical geographer, Donkin presents cartographic evidence as well as tabular statistics to show regional variations in wealth, forms of activity, and the scope of the economic catchment basins. My only criticisms are that some of the maps convey more than the layman's eye can readily assimilate and that the author is too laconic about the whole process of economic development, in which spiritual withdrawal became a smooth road leading to Christian enterprise and the bourgeois individualism of the separate Cistercian houses.

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WENDY R. CHILDS. *Anglo-Castilian Trade in the Later Middle Ages*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 264. \$23.50.

In my "Castilian Merchants in England, 1248-1350," in *Order and Innovation in the Middle Ages. Essays in Honor of Joseph R. Strayer* (1976), I pointed to the existing gap in our knowledge of this subject and the need for further study. With the publication of Wendy R. Child's book, the gap in our knowledge is now closed.

The book's first two chapters describe Anglo-Castilian trading activities between 1254 and 1485 within the framework of a fast-paced narrative of political history. Commercial exchange between the two realms rose sharply from the end of the thirteenth century to the ascent of the French-supported Trastámaras to the Castilian throne in 1369. As dynastic alliances shifted throughout the Hundred Years War, trade between England and Castile increased again to a high point in the late fifteenth century. Childs clearly shows how the merchants of both countries maintained their profitable ties, in spite of war and political discord. The nature of the political and religious conflicts of the sixteenth century, however, brought a promising commercial relationship to an end.

Chapters three and four consider the goods exchanged between the two countries. England's main export to Castile was cloth, with occasional exports of grain, tin, lead, tallow, feathers, and fish. In pointing to the growing importance of the English textile industry, Childs provides detailed descriptions of the types of cloth, prices, and origins. In return, Castile sent to England iron, wine, dyes, wool-oil, spices, olive oil, quicksilver, leathers, silk, and, under special royal dispensation, war horses. There is an interesting comparison of the economic development of both kingdoms in the

late Middle Ages, which in a sense determined the complementary nature of their trade.

The final three chapters explore the shipping, mercantile organization, and men involved in the trade. Most of the goods were carried by Castilian and English ships, and some of the Castilian sailors and merchants—mostly from the Bay of Biscay area and the interior cities of Burgos and Vitoria—also served as carriers of English goods to Flanders, Italy, and Gascony. Childs also examines the different weights and measures used, types of ships, commercial practices, patterns of investment, time involved in the sea voyage, insurance, credit, and every other aspect of Anglo-Castilian trade. She supports her conclusions with numerous examples and tables. Finally, the author gives an interesting analysis of the men involved in the trade, who in England were often office holders as well. From my research on Castilian merchants, I can verify that her conclusions also hold for Castile.

Although neither kingdom was the primary market for the other's goods, their trade, as Childs shows, was important and growing in the fifteenth century. In spite of some limitations in her use of Spanish archival material, her book is a solid, thorough, well-researched contribution to the field. It illuminates a topic that has been neglected until the present. Thanks to her we now know everything we should know about Anglo-Castilian trade in the late middle ages.

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GEORGES DUBY. *Medieval Marriage: Two Models from Twelfth-Century France*. Translated by ELBORG FORSTER. (The Johns Hopkins Symposia in Comparative History, number 11.) Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 139. \$10.00.

These lectures were delivered at Johns Hopkins University in 1977. Their main thesis is that there were in the twelfth century "two radically different and antagonistic models—the lay model of marriage, created to safeguard the social order, and the ecclesiastical model, created to safeguard the divine order" (p. 3)—and that these two models moved toward mutual accommodation as the century progressed. The best part of the book is Georges Duby's treatment of the lay model (sometimes, and more accurately, called the "aristocratic model"). The work mainly of heads of aristocratic households determined to continue the lineage and preserve the patrimony intact, it was well designed to serve those purposes.

Duby's presentation of the ecclesiastical model,

however, is muddy. The novelty of his treatment is that he is trying to describe the ecclesiastical model as it was actually applied in the twelfth century. Writings of clerical intellectuals and prescriptions of prelates are irrelevant except as they were invoked in specific cases. The method is not applied consistently, however, and the conclusions are confused.

For example, Duby says that the Church's model was monogamous. But he also says that the Church considered adultery or fornication to be grounds for divorce. The latter statement is simply not true unless accompanied by qualifications and explanations that Duby does not include. Furthermore, Duby follows the unfortunate example of countless other historians of nominalist disposition: when speaking of "the Church," he becomes an unabashed realist. The ecclesiastical model is the work of "the Church," but at times the Church is the pope, rather than the French hierarchy; at times it seems to be assorted chroniclers rather than the pope or prelates. At one point, the Church is represented by, of all people, Andreas Capellanus (p. 137, n. 42). The translator is swept from reification to personification when, in a single paragraph, the Church progresses from "it" to "she" (p. 22).

Duby's notion of rival models obscures the fact that much of the controversy surrounding twelfth-century marriages arose from the *absence* of a well-defined ecclesiastical model. The canon law and theology of marriage were unsettled throughout the century, and the worst part of the law was the prohibition of marriage between persons related to the seventh degree. The impossibility of consistently enforcing that rule meant confusion and embarrassment for clerics and laity alike. It also meant that aristocrats had a means of breaking almost any marriage and that clerics could use their power of adjudication and dispensation for their own political or financial advantage. Small wonder that in 1215 Lateran IV changed the rules on consanguinity (and affinity).

The documentation in this book is incomplete, inaccurate, and misleading. The worst examples appear in Duby's treatment of Ivo of Chartres. For example, two quotations attributed to Ivo are not to be found in the places indicated by the footnotes (p. 43, n. 66; p. 44, n. 67).

In my opinion, the views of Ivo are consistently distorted. The careful and well-informed reader can profit from this book; but, in general, it is likely to promote confusion and misinformation.

JOHN C. MOORE
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DEREK W. LOMAX. *The Reconquest of Spain*. New York: Longman. 1978. Pp. xii, 212. \$16.50.

Unlike some authors who would dispense with the concept of the reconquest altogether, Derek W. Lomax is firmly convinced of its reality. Of all the Christian lands occupied by the Muslims, Spain was the only one successfully reconquered by the Christians. The process extended over seven hundred years from the initial Muslim invasions in the eighth century until the fall of Granada in 1492. Lomax describes the establishment of Muslim rule, the vicissitudes of Asturias-León and the other Christian states that began to emerge in the northernmost reaches of the peninsula, the disintegration of Muslim Spain and the consequent progress of the reconquest, the intervention of the Almoravids and Almohads who forced the Christians to the defensive again, and the great reconquest of the thirteenth century. A brief chapter summarizes the wars of the late thirteenth, fourteenth, and fifteenth centuries and the final conquests of Ferdinand and Isabella.

The idea of the reconquest was clearly expressed in the chronicles of the reign of Alfonso III (866–911), which stressed that the kings of Asturias, as the heirs of the Visigoths, had the duty to reconquer the lands that the Muslims had illegally usurped. Sisnando Davidiz, the Leonese emissary to the king of Granada in the eleventh century, made the same point when he stated that the Christians intended to wear down the Muslims by tributes and then to expel them altogether. Until the early part of the twelfth century the reconquest was given a unitary character by the imperial title used by the kings of León, who tried to subordinate all the other peninsular rulers to themselves. But the rise of an independent Portugal, the formation of the Aragonese-Catalan federation, and the division of Castile and León effectively ruined the imperial idea. Even so the rulers of the Christian states understood that their cooperation was essential to the welfare of all and to the successful conquest of any Muslim territory. In the twelfth century the papacy began to take an interest in the reconquest and raised it to the status of a crusade, inviting northern Europeans to participate in it. Rather than depend on intermittent foreign assistance, the peninsular rulers chose to use their own resources and established the military Orders of Calatrava and Santiago and their affiliates as major instruments for carrying on the war in the late twelfth and thirteenth centuries.

This valuable little book will be particularly useful for students, for while it traces much of the military history of medieval Spain, it also discusses the important subjects of resettlement, the formation of towns, the growth of law, the establishment of churches, and the status of conquered peoples. Two points stand out as especially important. First, the reconquest was fundamentally a popular

war carried on almost continually by the people on both sides of the frontier, hardly interrupted even when kings from time to time negotiated truces. Second, late medieval Castile was a society organized for war. When the war was finished in 1492, it embarked upon the conquest, settlement, and Christianization of the New World, precisely the tasks for which it, alone among the Western European states, had prepared itself during the medieval centuries.

There are six excellent maps, a number of illustrations, and a good list of suggestions for further reading.

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J. VAN HERWAARDEN. *Opgelegde Bedevaarten: Een studie over de praktijk van opleggen van bedevaarten (met name in de stedelijke rechtspraak) in de Nederlanden gedurende de late middeleeuwen (ca 1300-ca 1550)* [Imposed Pilgrimages: A Study of the Practice of Imposing Pilgrimages (Particularly in Municipal Law Courts) in the Low Countries during the Late Middle Ages (ca. 1300-ca. 1550)]. (Van Gorcum Historische Bibliotheek, number 95.) Assen: Van Gorcum. 1978. Pp. xxv, 774. f 127.50.

This is a study of pilgrimages imposed by the municipal governments of the Low Countries as punishment for a large variety of crimes and misdemeanors. The study deals with the years between approximately 1300 and 1550 and concentrates mostly on towns in the southern provinces: Brussels, Antwerp, and Louvain, as well as lesser known towns. Tracing the imposition of pilgrimages to the *perigrinatio* and other religious antecedents of the pre-Carolingian era, J. van Herwaarden finds the origin of imposed pilgrimages especially in the conciliation between equals, a procedure for settling feuds that often included pilgrimages. The imposed pilgrimages consisted in travel to a shrine, confession, and prayer for self and the community.

The use of penal pilgrimages became frequent in the fourteenth century. The author suggests that at this time the condition of equality among the citizens was real and meaningful, and he also suggests a correlation between the increasing religiosity of the laity during and after the Black Death and the adoption of pilgrimages as punishment. He suggests that the decline in the imposition of pilgrimages—owing partly to the perennial suspicion of their efficacy—was caused by the greater social stratification of municipal populations in the sixteenth century and by the consolidation of sovereign jurisdiction under the Burgundian Habsburgs.

A significant part of the author's research deals with the growth of distance tariffs that helped in determining financial composition of the imposed pilgrimage on the basis of a *per diem* cost of travel and expenses. The author has not found any connection between the saint of a particular shrine and the crime for which the pilgrimage was imposed. There seems, however, to be some correlation between the seriousness of the crime and the distance of the pilgrimage. (Could it be that the distance was determined by the wealth of the criminal, and thus by his social position? This is suggested by the fact that ability to bear the cost appears to have been a factor for the court.)

This is an important contribution to the field of legal, social, and religious history, and the author deserves praise for his examination of the many published materials that he carefully checked and expanded with archival research. It is to be hoped that doctoral candidates will pursue the areas indicated but justifiably unexplored in this work.

DERK VISSER
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DUŠANKA DINIĆ-KNEŽEVIĆ. *Položaj žena u Dubrovniku u XIII i XIV veku* [The Position of Women in Dubrovnik in the Thirteenth and Fourteenth Centuries]. (Posebna Izdanja, number 469; Odeljenje Istorijskih Nauka, number 2.) Belgrade: Srpska Akademija Nauka i Umetnosti. 1974. Pp. xvii, 223.

This monograph on the position of women in medieval Dubrovnik (Ragusa) by Dušanka Dinić-Knežević, published by the Serbian Academy, will appear to a Western audience to be a response to the wave of feminism that has had such a marked influence on Western scholarship. Scholars in a socialist nation such as Yugoslavia would tend to deny this conformity to a trend, however, emphasizing, with at least a degree of accuracy, that dispossessed groups have always received their share of historical attention and that women scholars have been well integrated into the academic ranks in a fashion unknown in the West until recent years. Dinić-Knežević, professor of medieval history at the University of Novi Sad, is one of a group of Yugoslav scholars working in the Dubrovnik archives and producing some of the best archival studies currently being written. The work is in Serbo-Croatian and will, for that reason, fail to receive the attention it deserves. It is a topically organized and orderly exposition of the role of women as revealed in statute law and the charters, for the most part emphasizing the documented decades of the very last years of the thirteenth and the fourteenth centuries. This is Dinić-Knežević's field of specialization; she has previously pub-

lished articles on the trade in wheat, wine, the Black Death, and other social and economic topics.

Chapters concentrating on women and the economy, women and marriage, morality, and spiritual life deal primarily with urban women, with the greatest detail and, therefore, emphasis upon the lives of the women of the noble class. Affixed to each chapter is a shorter section on the lives of women in medieval Serbia. It can be easily understood that Dinić-Knežević wished to expand her study beyond the limited perspective provided by one archive, but the paucity of legal materials, in fact of Serbian data of any sort, beyond the Code of Dušan and a few scattered lives of queens and saints, provides a rather unsatisfactory contrast to the rich detail of the lives of the urban women of Dubrovnik. This creates a distortion in the importance of Serbian women to their own society; only ethnography, the investigation of folk literature such as laments and sagas, can do these women justice. In this monograph they tend to pale through comparison. Still, as a research tool the book is valuable: if legal data has survived on Serbian women, this work will introduce the reader to it.

Women of the town are an entirely different matter. Dinić-Knežević has made good use of wills, pious bequests, civil indictments, sales, contracts, and dowries to understand their lives. A strong emphasis on class relationships characterizes the study, with the tacit assumption that class lines had the same significance for women as they did for men, a highly debatable issue. Possibly the most valuable sections are the small biographies or vignettes of outstanding women, such as the life of the vigorous and ambitious heiress Domina Philippa de Mence and the sadder life of the pious Nicoletta de Sörgo. While women in Dubrovnik were permitted to hold only one office, that of *salinarius* or inspector of the salt drying pans, the author is properly aware that the social and economic power of certain aristocratic women outran the limits of the domestic sphere, to the evident satisfaction of all concerned, just as long as the needs of the aristocratic family were served. This is the juncture where a comparative perspective with other medieval urban centers would have been valuable. Unfortunately Dinić-Knežević has only a limited awareness of the more recent work done on the social history of such major towns as Florence, Genoa, and, particularly, Venice. In fact besides Buckler's 1936 study of women in Byzantine law and Bardeche's *L'histoire des femmes*, few, if any, of the works of Western scholarship on medieval women are cited. Such studies might have inspired a quantification of dowry bequests in comparative perspective, for example, to see if dowry inflation

ran the same course at Dubrovnik as it did in other commercial centers.

Both methodologically and in comparative perspective the studies of scholars working in other archives would have proven useful to the author. Given the fine collection of data that Dinić-Knežević's superior archival research techniques make available, much more could be done in the way of synthesis. Still, the painstaking piecing together of evidence on the lives of medieval women, using different series of records, provides highly valuable data on the history of urban women for the reader who has a grasp of Serbo-Croatian.

The book contains a summary in French, which, however, does no more than provide a glimpse of the amount of data in the text.

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LAZAR MIRKOVIĆ. *Minijature u antifonarima i gradualima manastira sv. Franje Asiskog u Zadru* [Miniatures in the Antiphonaries and Graduals of the Monastery of St. Francis Assisi in Zadar]. (Posebna Izdanja, number 501; Odeljenje Istorijskih Nauka, number 4.) Belgrade: Srpska Akademija Nauka i Umetnosti, and Filozofski Fakultet u Beogradu—Institut za Istoriju Umetnosti, Belgrade. 1977. Pp. 92, 82 plates, xxvii.

In this book Lazar Mirković examined eight illuminated manuscripts (antiphonaries *H*, *C*, *F*, *G*, and *E* and graduals *A*, *B*, and *D*) preserved in the monastery of Saint Francis of Assisi in Zadar, Yugoslavia. The study focuses on visual, rather than on musical, elements in these codices, and therefore, presents a detailed, formal analysis of the large initials formed of floral and geometric ornaments containing either single human figures or figurative compositions. After a general description of the codices, it examines the image of each folio containing a figurative initial. This is followed by iconographic analysis and citation of parallels or by identification of a given scene or figure. In some cases Mirković corrected erroneous identifications by scholars who previously dealt with some of these miniatures and offered some brilliant solutions to problems of medieval iconography (pp. 16–17, fig. 20). Unquestionably knowledgeable about the liturgy and other theological texts, Mirković elucidated, in most cases, the relationship between the image and the text that inspired it. In this area of text-image relationship lies the most valuable contribution of Mirković's study. Questions arise, however, from some of his iconographic interpretations. Mirković tied the unusual image of a crowned Christ writing (pp. 8–9, fig. 7) to the

iconographic model of the writing evangelist, John, without considering that of King David, author of the Psalms.

The descriptions of both groups of manuscripts are followed by short stylistic analyses through which the author suggested a likely date and locality of origin: for the antiphonaries, the late thirteenth century and the Veneto-Paduan region, and for the graduals, the early fourteenth century and the Bologna workshops. His conclusions seem to be based predominantly on style, although the practice of compilation of sources available to the scriptoria and appropriate to a given book make style less than reliable unless combined with paleographic and other type of evidence.

The word "Byzantine," as used by Mirković, should be understood as "Byzantinizing." Further research is necessary to determine how Byzantine are the styles in these manuscripts, especially in the antiphonaries. The basic iconography in a majority of cases is obviously Byzantine but with a strong overlay of Western iconographic and stylistic traits.

Published nine years after Mirković's death, this book is visually handsome and very readable, with sharply reproduced illustrations, regrettably only in black and white. It is the first comprehensive study of the Zadar antiphonaries and graduals, and it provides a sound basis for further research, especially on the possible number of masters and on the style of the miniatures.

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MODERN EUROPE

JEROME BLUM. *The End of the Old Order in Rural Europe*. Princeton: Princeton University Press. 1978. Pp. xiii, 505. Cloth \$27.50, paper \$12.50.

Between 1771 and 1864, thirty-eight European states issued decrees emancipating their peasantry from servitude. The first in line was Savoy, the last Rumonia. Jerome Blum tells how this came about, first by filling in the background of the traditional order (seigniors and their privileges, peasants and their obligations, agriculture and its retardation), then by showing how the old servile order was sapped by reforms (and talk of reforms), new directions in agriculture, and peasant unrest, and finally by chronicling the emancipations themselves, their circumstances and their effects on peasants, nobility, economy, and polity as a whole.

The scope of the work is vast, ranging in time from the seventeenth century to our own (its core

is in the eighteenth and nineteenth centuries) and in space from Brittany to the Urals (though most Mediterranean lands receive little or no attention). Its pages are packed with information, their sweep somewhat reminiscent of Robert Palmer's *Age of the Democratic Revolution*. Blum tells us that emancipation came as the result of many factors—above all, the determination of central authority to assert itself and strengthen its power at the nobles' expense. The thesis that the needs of burgeoning capitalistic production had anything to do with it is given short shrift. For Blum the reforms that freed peasants from servility were "the last great triumph of royal absolutism over nobility."

But these revolutionary actions, all coming from above, took time to become effective. The book contains fascinating sections on the qualifications and delays imposed on the would-be reforms. Thus, in Savoy, seignorial privilege and peasant servitude were abolished in 1771 but not really ended until the French occupation in 1792. In Baden, the delay between statement and implementation was sixty-five years (1783–1848); in Denmark, where serfdom was abolished in 1788, its traces persisted until 1861. In many German lands seigniors kept their court and police powers until the 1860s and 1870s. And in most countries outside France the onetime serfs (a majority of the population) continued to suffer from unequal treatment in the realms of taxation, punishment, suffrage, freedom to travel or settle, as well as from unequal opportunities—for instance, in access to credit or education. The social and political inferiority of the peasantry persisted after emancipation, as did the traditional superiority of the nobles. But the latter gradually declined in all lands and the former gradually improved in most so that, by 1914, Europe's eastern borderlands apart, the old rural order had indeed come to an end.

I cannot resist a few observations on France: Paul Bois's evidence for western France does not appear to bear out the assertion that peasant *cahiers* "were filled with complaints and anger at . . . servile obligations and seignorial privileges." Seignorial interest in pigeon cotes rested less on a penchant for pigeon pie than on the sport the birds could offer and the use of their droppings to feed manure-starved lands. The widespread use of *métayer* and *métayage*, not only for sharecropped lands but also for those held on lease, can easily increase the count of sharecroppers. Slash-burn tillage, which we are told had gone out in White Russia and Lithuania by the second half of the nineteenth century, could still be found then in small parts of France. On a more general plane, although Blum is careful and fair in apportioning responsibility for agricultural backwardness, his attribution of part of this to peasants nettles me slightly. He is

right to complain that peasants clung too long to old, inefficient ways, did not invest additional income in improvements (no more, too often, did the bourgeoisie), and remained outside the market nexus. But he does not ask what choice they had or suggest what course of action available to them in practice would have served them better.

In the impressive context of this big book, however, this sort of thing is niggling. Blum has given us a compendious, learned, and useful work, which all historians of modern Europe can read with profit.

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NORBERT ELIAS. *The Civilizing Process: The History of Manners*. Volume 1. Translated by EDMUND JEPHCOTT. New York: Urizen Books. 1978. Pp. xviii, 310. \$15.00.

This is a translation of volume one of *Über den Prozess der Zivilisation*, published in 1939 and written over forty years ago (the preface is dated 1936). The two volumes were republished in 1968 with a new preface by Norbert Elias, which is included as an appendix to this volume, but if there were changes in the original text they are not included here. Elias was concerned with the concept of *civilité*, or civilization as a "specific transformation of human behavior," in the period between 1400 and 1800, primarily in France and Germany, to a lesser extent in England and elsewhere. He argued that there were and are long-term changes in the affect and control structures of people in particular societies, changes which follow one and the same direction over a large number of generations. The second volume, to be published later in English, argued that changes in society give rise to changes in personality structure.

As documentation for his thesis Elias examined aspects of manners such as table behavior, eating utensils, the proper way to blow one's nose, spitting, controlling flatus, and changes in bedroom behavior, as well as changing concepts in the relationships between the sexes. His sources were primarily books of manners from the fifteenth-century *Babees Book* to the eighteenth-century *Les Règles de la bienséance et de la civilité chrétienne* (La Salle), but he included references to other works written both before the fifteenth and after the eighteenth centuries. Erasmus's *De civilitate morum puerilium* served as a sort of bench mark by which he measured changes. Quotations from the works are usually given in English in the text with foreign language originals included in an appendix. The book is footnoted and there is an index.

Though Elias was careful to emphasize that he

was not depicting any automatic progress of manners and looked at some variables, particularly the class of the authors, the intended audience, the extent of literacy, and the power of the court, his sources were selective. Not all books of manners were included, and it is not clear why one book was chosen and others ignored. A further weakness of the book is that he ignored variables such as the emerging concepts of privacy made possible through changes in building techniques, the changing status of women, medical and scientific discoveries, and the influence of technology, which either collectively or individually might have given a different explanation for some of the changes that he viewed primarily from the various books of manners. His selections illustrated his thesis that changes took place in the direction of advancing differentiation and integration, but how much, for example, was this due to technology? Would the fork have become so ubiquitous if the techniques for its manufacture had not been invented, and which appeared first, the techniques or the demand? Was the public attitude toward expectoration due to a medical recognition of some of the dangers, or was it simply a matter of what constitutes good manners? Like most thesis works, questions that do not easily fit into the overall schema are ignored. Still, since no more detailed history of manners and bodily propriety has yet, at least to my knowledge, been written, the publisher and the editors of this book, who are devoted to establishing a theoretical basis for social, intellectual, and cultural change, are to be commended for translating and publishing it.

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ALLEN G. DEBUS. *The Chemical Philosophy: Paracelsian Science and Medicine in the Sixteenth and Seventeenth Centuries*. In two volumes. New York: Science History Publications. 1977. Pp. xv, 293; 295-606. \$60.00 the set.

The general thesis of these volumes is that there flourished in the sixteenth and seventeenth centuries a distinct "chemical" philosophy, believed by its adherents to offer a uniquely Christian and uniquely effective approach to nature and health. Although they themselves used much from the past, these philosophers were forceful in their iconoclastic attacks on the idols of the scientific and medical establishment, Aristotle and Galen.

Vitalistic, sometimes gnostic, sometimes cabalistic, sometimes Neoplatonic or Neopythagorean views informed this philosophy. Its followers usually rejected the new Copernican astronomy, as well as rational mathematics and logic. They

raised a great hue and cry for "experience" (as opposed to reliance on ancient texts), yet they produced no clear prescription for experimental methodology, and they offered more peculiar explanatory devices than Aristotle or Galen ever did. What then is the value of studying them? Debus argues that we need to seek "the totality of scientific and medical themes . . . in the literature of this period" (p. 539), with which position the present reviewer agrees. To study only the scientific and medical ideas that "won" is to leave ourselves with an impoverished view of the complexities of intellectual achievement. The "correct" answers were never obvious before hand.

These volumes then are valuable steps in the right direction, and they contain a wealth of information and bibliographic detail. But they cannot be the final word on the subject. The finer points of difference among the chemical thinkers are submerged in the effort to establish the existence of their general point of view. No individual system of chemical thought is ever stated as a coherent philosophical position with its own set of assumptions and the kind of internal logic that would make its intellectual credibility apparent to the modern reader. And although a general chronological framework seems to have been intended, that frequently breaks down as the arguments of different writers are reviewed. One result is that the chemical philosophy never seems to rise to the level of a continuous, progressive, intellectual dialogue. Did it so rise, or not? We do not yet know. The author asserts "that a fundamental change occurred in the century and a half that separated the student years of Paracelsus from the earliest publications of Robert Boyle" (p. 540)—implying just such continuity with development—but the reasoning from historical relationships that would give substance to his statement is largely lacking.

A large proportion of these two volumes is based on work that had previously appeared in print, and many sections retain the characteristics of isolated studies. Yet, however tenuous their common focus is, it lies in the general chemical philosophy, long neglected by historians, the nature of which, and indeed the very existence of which, is brought to our attention by this work. That the chemical philosophy was seen in the sixteenth and seventeenth centuries as a viable alternative to the systems of antiquity, and also to the new mechanism, now seems established beyond doubt.

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DENYS HAY. *Annalists and Historians: Western Historiography from the Eighth to the Eighteenth Centuries*. London: Methuen; distributed by Barnes and Noble, New York. 1977. Pp. viii, 215. \$16.50.

In this short, clearly written book Denys Hay has performed a valuable service both for students of historiography and for teachers of the subject. A treatment of this period is particularly welcome, since these centuries produced many important historians who are seldom read today. Hay's book is superior to such general works as Thompson's or Barnes's because he has a more sophisticated framework for organizing the material. Instead of simply listing forgotten historians and unread works, he traces the development of modern historical writing. Hay argues that neither classical nor Biblical writing ever produced a good means of describing the distant past. Classical historians, lacking an adequate chronological apparatus, were interested almost wholly in the immediate past, while Biblical and early Christian historians were too preoccupied with the transcendental meaning of history. Modern chronology grew out of early Christian attempts to develop long-term Easter calendars. The author clearly describes how the calendar emerged and how it, together with the Biblical sense of continuity in history, produced the medieval chronicle. This form differed from earlier historical writing in that it contained a summary of world history as well as contemporary material arranged annually and dated from the Incarnation.

The Renaissance contribution to this development was twofold. First, the discovery of anachronism produced antiquarians who were interested in reconstructing the distant past as accurately as possible. Second, the Renaissance imitation of the classics produced literary historians who sought primarily to entertain their readers. This split continued through the seventeenth century, when scholars developed important new techniques of research and chronological arrangement, while literary historians continued to write entertaining works with no particular concern for accuracy. The two traditions merged only in the eighteenth century into what Hay calls a "mature approach to the history of earlier times" (p. 170).

Hay's conceptual framework is intriguing and makes considerable sense of the seventeenth and eighteenth centuries, allowing the reader to find an order there that is seldom present in general works on the subject. Yet his thesis tends to obscure many important elements of the medieval and Renaissance historical traditions. In particular, by emphasizing the chronological interests of medieval writers he misses the rich classical influences in medieval historical writing. These medieval chapters could have profited from more attention to recent work in the field, especially that of R. W. Southern. Hay also exaggerates the importance of periodization in the narratives of practicing humanist historians other than Biondo and overlooks the imagination and flexibility with which the best

of them adapted their classical models to their own needs.

The book's emphasis on chronology and research recalls the positivistic approach of Fueter, for whom the author has well-justified respect. But the approach is often unsympathetic to the goals of previous historians, as is obvious in Hay's off-handed remark that "most classical historians are bad historians—or perhaps one should rather say that they were attempting to do something completely different from what is now regarded as the historian's task" (p. 1). Indifference to goals other than accurate description of the past leads the author also to ignore the conceptual problems behind that goal. After remarking that the research of the seventeenth and eighteenth century historians finally put Pyrrhonism to rest, he adds in a footnote, "There are, needless to say, still those who hold that 'History is bunk'" (p. 170). This remark reflects both the strengths and weaknesses of the book. It is a welcome addition to the literature in the field—elegant, witty, and clear but somehow evasive in its treatment of major historiographical issues.

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JEROME FRIEDMAN. *Michael Servetus: A Case Study in Total Heresy*. (Travaux d'Humanisme et Renaissance, number 163.) Geneva: Librairie Droz. 1978. Pp. 149.

In thirteen succinct chapters, Jerome Friedman introduces the life and thought of "the complete heretic" of the sixteenth century, Michael Servetus, who repudiated Rome, Wittenberg, Geneva, and Zürich, along with infant baptism, Christ's humanity, original sin, and the orthodox trinity. Servetus was in turn repudiated by the Catholic Inquisition and burned in Geneva after a celebrated trial, which in turn initiated a toleration controversy and furthered the transition toward freedom of thought in the modern age.

Having examined previous Servetus scholarship (Bainton, Williams, Manzoni, Kot, and Fernández), Friedman uses five of his own previously published articles to round out a newly written, systematic, topical discussion of Servetus's method, theology of God and man, sacramental teaching, and use of sources from scripture, patristics, rabbinics, and Neoplatonism. Friedman writes in a rationalistic manner without revealing great sympathy or antipathy for his hero, while at the same time his grasp of traditional theological orthodoxy is acceptable and trustworthy. The result is a balanced, well-argued, and convincing description of a most confusing, eclectic, and autonomous arch-heretic.

The author weighs many influences upon Servetus, including his openness toward Jewish thought and cabalistic mysticism, but concludes that Servetus was not so much a judaizer, hellenizer, or esoteric figure as he was a scriptural interpreter who found modalism to be necessary for salvation. In turn, he was able to combine Jewish, patristic, and pagan sources into an amalgam that he thought verified early Christian truth through the universally implanted wisdom of the spermatik logos. The outcome of all of this, Friedman concludes, was "variously a messianic form of Neoplatonic Judaism, a free will oriented Gnosticism, or a generally hellenistic Christianity seemingly permeated with heresy." Friedman thinks that Servetus's system was successful insofar as he could demonstrate integrated wholeness to his views, a conclusion that some of the readers of his book might question. Friedman does demonstrate that Servetus's total opposition to all of the communities of his day forced him to be a heretic by any and all standards.

In a more tolerant age, it would be interesting to reconsider the worth of Servetus's views, which Friedman declines to do, staying safely within the limits of his immediate historical responsibilities. One senses that Friedman finds Servetus useful today for intellectual and religious purposes, although he never says that. In the light of his balanced and careful treatment of Servetus, for which persons interested in sixteenth century heresy will be most grateful, Friedman might fairly be requested to do an additional apologia for Servetus. Taking into account all that Friedman has to say about Servetus in his fine book, the reviewer's judgment is that Servetus's reputation as a confused and unsavory, although very brave, heretic continues to be correct.

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JEAN-PIERRE LABATUT. *Les noblesses européennes de la fin du XV^e siècle à la fin du XVIII^e siècle*. (L'Historien.) Paris: Presses Universitaires de France. 1978. Pp. 184.

Jean-Pierre Labatut of the University of Pau is already known as the author of *Les ducs et pairs de France au XVII^e siècle* (1972). In the present volume, written for Roland Mousnier's series, *L'Historien*, he has moved from a specialized treatment of one level of nobility, in one country and in one century, to a broadly comparative overview of Europe's "second order" over a period of some three hundred years. The result is a concise work of useful knowledge, presented with the lucidity one expects from the best of French scholarship.

The structure of Labatut's book is clear-cut. In

part one he discusses general problems, practical as well as theoretical, having to do with ranks, titles, and hierarchical relationships. Part two concerns values and attributes of the early modern nobility, rooted as it was in the Middle Ages: birth and family, honor and military service, the claim to political power, and the range of economic assets. Then in part three we are invited to consider the complex issue of aristocratic unity versus national interests and peculiarities. This leads to some interesting remarks on the interaction between supranational influences—such as the roles of the Order of Malta and the less frequently cited Baltic knighthood—and on princely tactics within particular states. The section concludes with reflections upon courts, careers, intellectual life, and the art of living well: in short, upon an “aristocratic civilization.”

There is throughout a heavy emphasis on France. Earlier French texts dominate the brief bibliography; and in the list of modern studies cited, twenty deal with France, two each with Italy and the Low Countries, three with Russia, six with England, and so on. Still, given the centrality of the French case, from Merovingian to modern times, this familiar tendency is here perhaps especially easy to explain and to forgive. It should also be said, in fairness to the author, that he is consistently attentive to English, Spanish, Italian, Polish, Russian, and other significant variations.

Occasionally—and this is my only reservation about the book—Labatut seems eager to set up straw men among seldom-named historians and then to knock them down with arguments no longer really needed. For example, at several points he insists that it is a serious oversimplification to portray the old, military nobility of France in the eighteenth century as either totally isolated from or abjectly merged with the *noblesse* more recently arisen from the ranks of judges, administrators, and financiers. True, but who in the last generation or so has offered any such picture, a simplistic one indeed? At another juncture, he defends the high nobility during the age of the Enlightenment against charges of intellectual torpor by unnecessary references to the ample libraries of many dukes and peers of France—not, it is curious to note, by recalling such names as Montesquieu, Shaftesbury, Beccaria, Linnaeus, or others belonging to the considerable body of titled *philosophes*.

These, however, are only minor complaints. Part of the historian's task is to observe and to explain the social, political, economic, and cultural role of elites—elites of power, not just of privilege—their formation, influence, rigidification, and decline in the face of other elements that had become more energetic, ruthless, sometimes lucky, but nevertheless elite themselves. The stubborn reality of

political control by relatively small groups in all ages and in all known societies, defying absolutist theories and expectations both monarchic and egalitarian, is one of the great challenges to non-partisan understanding of the human condition. It demands, no doubt, a quest for knowledge extending, both geographically and chronologically, far beyond the experience of early modern Europe; but in organizing that crucial segment of the record, Labatut has performed an important service for us all.

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ALWIN HANSCHMIDT. *Republikanisch-demokratischer Internationalismus im 19. Jahrhundert: Ideen—Formen—Organisierungsversuche*. (Historische Studien, number 430.) Husum: Matthiesen Verlag. 1977. Pp. 111.

Since Marx urged workers of the world to unite over a century ago, internationalism has often seemed the prerogative of socialism. Less renowned are the attempts of middle-class radicals in various countries to coordinate their activities between 1792 and 1849, and down to the twentieth century. Although these efforts failed, they inspired the proletarian solidarity sought by Marx and his successors in a series of Internationals. In this instance, as in many others, bourgeois mentors guided the labor parties of Europe toward political maturity. Alwin Hanschmidt elucidates this relationship by tracing the development and explaining the weaknesses of republican-democratic internationalism in the nineteenth century.

Following Hans Kohn, Hanschmidt writes that “in the relationship between nationalism and internationalism . . . there occurred a thoroughgoing change about the middle of the nineteenth century” (p. 9). He concludes that, for two reasons, the years 1848–49 initiated a long period of decline in bourgeois internationalism: (1) during the second half of the century nationalism lost its identification with popular sovereignty and became instead a bulwark of the authoritarian state and (2) democrats lacked the sort of overarching issue found by socialists in the class struggle. Bourgeois radicals continued, moreover, to propose political and constitutional solutions to social and economic problems long after socialists, at least in theory, had abandoned reform for revolution. A commitment to gradualism and to the sanctity of private property prevented an alliance between democrats and the working class. By the 1870s bourgeois internationalists were espousing the ideals of peace and freedom in an increasingly hostile world. The republican visions of an aging

Mazzini had replaced the robust activism of 1848–49.

Hanschmidt sketches this process in a clear and economical manner. His study displays several of the virtues of brevity, while demonstrating a few of its drawbacks. Readers will therefore come away informed, if not entirely satisfied. The author's reliance on published works, with only an occasional foray into the primary sources, produces some discomfort. A second criticism, which would admittedly require a longer book to meet, also seems worth mentioning. Hanschmidt asserts correctly that the nonrevolutionary democrats sought in vain for political answers to the "social question," but he does not delve deeply enough into the writings of bourgeois radicals in the 1830s and 1840s. Why were the democrats unable, during later decades, to address basic issues whose importance they themselves recognized? The author does not attempt a thorough analysis of this question, calling instead for additional research into the matter.

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MARILYN J. BOXER and JEAN H. QUATAERT, editors.
Socialist Women: European Socialist Feminism in the Nineteenth and Early Twentieth Centuries. New York: Elsevier. 1978. Pp. x, 260. Cloth \$15.95, paper \$9.95.

In eight essays, this book provides sketches of the lives of the most prominent socialist women, beginning with the French utopian Flora Tristan and ending, one hundred years later, with Austrian Social Democrats Adelheid Popp, Therese Schlesinger, Gabriele Proft, and Emmy Freundlich. The distance between Tristan's early attempts to deal with female emancipation from a socialist perspective and the Austrians' comprehensive program of reforms for women is bridged by studies of the Russian Populists, French Workers' Party, German Social Democrats, Italian Socialist Party, and Bolsheviks. The theme tying all these essays together is the attempt to generate an authentic socialist feminism, a strategy of approach to proletarian women, and a program of reforms for them that would translate the socialists' declarations of support for women's emancipation into practice. The purpose of the collection, in the words of its editors, is to "explore the strengths and weaknesses in the alliance of socialism and feminism as it was forged in the nineteenth and early twentieth centuries" (p. 3).

The introduction by Marilyn J. Boxer and Jean H. Quataert defines the differences between bourgeois and socialist feminists and sketches, a bit too briefly, the socialist theory on the woman

question as developed by Engels and Bebel. A fuller, more sophisticated exposition of ideology would have explained the limitations of economic determinism in accounting for gender-based discrimination and dealt with the issue of separatism versus unity in the socialist movement. A common ideology unites most of the women studied here and that ideology generates the same difficulties for all, despite differences of nation and time. Thus, its strengths and shortcomings should be fully discussed at the outset.

The essays that follow are generally lucid and enlightening. The authors are sympathetic to their subjects, but they also consider culture, working-class attitudes, and issues of *Realpolitik* that shaped and limited the socialist commitment to women. Marilyn Boxer's essay on the French party is particularly strong in this regard. The collection is further enriched by the drama of the lives of its subjects. Flora Tristan overcame illegitimacy to rise to prominence as a socialist agitator. Madeleine Pelletier, a physician, died in a mental institution where she had been confined for practicing abortions. Anna Kuliscioff left her native Russia as an exile, adopted Italy as her homeland, and was hailed by her comrades as "the most intelligent man in Italian Socialism." Perhaps the spirit of these women and the difficulties they faced are captured best by Barbara Alpern Engel in an essay on the Russian Populists. She analyzes the issue of socialism versus feminism, but she also deals with the lives of her subjects vividly, and thereby she manages to combine ideology and personalities into a well-developed, graceful essay.

Socialist Women is useful to anyone studying women's history or the history of socialism. Although not always as sophisticated as one might wish, it is a coherent attempt to deal with socialist pronouncements and practice in the area of female emancipation.

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ČEDOMIR POPOV. *Francuska i Srbija, 1871–1878* [France and Serbia, 1871–78]. (Posebna Izdanja, number 473; Odeljenje Istorijskih Nauka, number 3.) Belgrade: Srpska Akademija Nauka i Umetnosti. 1974. Pp. 447.

In the 1870s Adolphe Thiers believed the Czechs to be "Germans with a particular dialect." Jules Favre queried Rieger, "The Czechs are Russians, aren't they?" Leon Gambetta thought that the Rumanian masses lived in Macedonia. And Victor Hugo, describing Bulgarians in 1876, cried, "Vive la Serbie!" Balkan problems were usually unknown to the rest of Europe, except, to a certain

degree, those of the Greeks. French policy in South Eastern Europe during the 1970s was shaped by three factors: the European balance of power (including the Anglo-Russian and Austro-Russian rivalries), the French confrontation with the German Reich, and movements for national emancipation in the Balkans, which threatened to jeopardize the established order.

Cedomir Popov is aware that his study cannot offer something dramatically new about the general development of French eastern policy after the defeat in the Franco-Prussian war. In essence, this policy was based on the preservation of the status quo in the Balkans and the safeguarding of French advantages already achieved in the Ottoman empire and with respect to the Eastern Question. Entering into the details and nuances of French policy during the Eastern crisis, he proves that the Third Republic cherished the same motives as the Second Empire but changed the strategy and methods. The Eastern Question always represented a "major problem for French interests in Europe, second only to those of her frontier." The appearance of the new German Reich, however, required that "these problems be handled with extreme prudence" (p. 79). This prevented major initiatives, but ensured the French presence in all Balkan contemporary matters. The author estimates that such behavior, although essentially negative, was pragmatic and brought to Serbia more advantages than harm.

The book is divided into two parts, covering Franco-Serbian relations from the Frankfurt peace until the outbreak of the Eastern Crisis and the crisis itself. The first part includes chapters on France's domestic situation (1871-75), its international position, and its attitude toward the Eastern Question and Serbia. Part two deals with general problems facing French diplomacy during the eruption and development of the Eastern crisis, the activity of French diplomacy in the Balkans and Serbia, French behavior during the two wars in 1876 and 1877, and, finally, France's role at the Berlin Congress.

Besides being a well-documented and detailed diplomatic history of Franco-Serbian relations during the Eastern Crisis, Popov's study has, in my opinion, an importance for Yugoslav historiography in general. The author views Serbia from Europe and not vice versa, as is usual in Yugoslav historiography. He offers an opportunity to consider Balkan events as part of larger European diplomacy. Popov is a disciple of two historical schools, one developed in Yugoslavia, the other, in France. He is both a Marxist and a follower of Pierre Renouvin, whose student he was during his studies in Paris. The result is a combination of nondogmatic Marxism and the methods applied

by Renouvin in his *Introduction à l'histoire des relations internationales* (1964). Popov rejects the "apriorism of so-called contemporary post-Marxist historiography," but accepts Renouvin's historical factors only in their dialectical correlation.

The book is based on an extensive use of primary sources taken from French and Serbian archives. It is a pity that a bibliography of sources is not published at the end of the book.

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FRIEDRICH WÜRTHLE. *Dokumente zum Sarajevoprozess: Ein Quellenbericht*. (Mitteilungen des Österreichischen Staatsarchivs, Ergänzungsband number 9.) Vienna: Ferdinand Berger und Söhne. 1978. Pp. 132. \$16.00.

Books have their fate, and so do documents. But few can have had as dramatic a fate as the documents involving the Sarajevo assassination. Many were lost or destroyed; others were "borrowed" by private individuals; and whole sets of archives were removed and only partially returned. Large portions of the police interrogation of the arrested assassins are gone, as are the protocols of the judicial investigation. The two court stenographers in the trial of the assassins took their day's work home with them at night, though the surviving stenographer later tried to reconstruct the record from partly illegible notes. Most of the Serbian diplomatic archives were evacuated in World War I, as the Austrians were advancing on Belgrade, but the Austrians captured some, as did the Bulgarians, while others were burned or lost. Those that found their way back to Belgrade were captured once more in World War II, this time by the more thorough Germans (clichés can be true), who put a historical research team, with headquarters in Vienna, in charge of editing some of the documents. The first volume, which dealt with Sarajevo, was ready for the printer at the end of 1944. It was an inauspicious time for scholarly publishing, and only a small, forty-page selection from that volume actually saw print, early in 1945, and even that quickly became a collector's item. About the only document that survived unscathed, and in unimpeachable form, has been the trial indictment. So courage is needed to deal with Sarajevo—and that is the kind way of putting it.

The person who deserves the credit for putting all this information together is Friedrich Würtle, who died in 1976, before he could quite finish this volume. He was an endearing person, and this is an endearing book, quintessentially Austrian in reconstructing what must of necessity remain frag-

mentary; all this labor to erect a ruin. Würthle was what can be called a historical amateur in the best sense of the term. He began his career as a journalist and an author of children's books, wrote plays, and after 1945 served first as press secretary to the Austrian chancellor and later as press attaché in Bonn. But an interest in Sarajevo grew into a near obsession, and he established his own Sarajevo Research Archive, which his untimely death, alas, kept him from indexing entirely. Still, this is a good book to have. The scholar in the field may find nothing that is new or startling, but it surely helps to have this dispassionate, and thorough, examination of the existing evidence, as well as an explanation of precisely what was lost, and how. If we had more amateurs like this, these would be better days for the profession.

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JEAN VAN HEIJENOORT. *With Trotsky in Exile: From Prinkipo to Coyoacán*. Cambridge: Harvard University Press. 1978. Pp. ix, 164. \$12.50.

This is a tantalizing and disappointing volume. It is, in sum, a chatty, almost gossipy account, written largely in short declarative sentences, and lacking in either subtlety of style or substance. In it, Jean Van Heijenoort treats us to a series of vignettes, episodes, and events in the daily routine of the Trotsky entourage. There is precious little in the way of explanation for these events or of their context, and perhaps most important, only rare efforts at explaining Trotsky's views or positions with regard to them. Perhaps the most frustrating aspect is that Van Heijenoort notes that Trotsky broke relations with some individual or group without supplying any reason for the rupture. Or, if an explanation is given, it is a highly subjective one (a conflict of personalities, some fault or flaw of the other party).

On the other hand, the author does provide us with a wealth of information of dubious value (the disciplining of dogs, outbursts against noise, the size of Trotsky's engorged member on contemplating a reunion with his second wife Natalia Sedova). And, while a number of these may be of passing interest to the general reader, they do not constitute a careful or thoughtful presentation of Trotsky's life in exile.

These several complaints could simply be the result of Van Heijenoort's position in the Trotsky retinue. He does not, from his own testimony, appear to have been more than a private secretary/messenger for Trotsky, employed either in translation, dictation, or in the dispatch and execution of Trotsky's directives to Trotskyist groups.

He cites no instance of substantive disagreement between himself and his employer (unless one counts his belated objection to Trotsky's comparing the loss of power with the loss of a wallet) and seems to have maintained a more personal than political relationship with Trotsky and Trotskyism. The personal nature of his commitment would, at any rate, explain both the curious *in camera* nature of this volume (events take place in a mildly paranoid and detached environment) and the author's subsequent break with Trotskyist groups (following World War II). It would also explain why the best, or most vivid, passages in this book are those dealing with the more personal aspects of Trotsky's activities: the suicide of his daughter Zina; the death of his son Leva (and the possibilities of KGB involvement); Trotsky's often farcical sojourn in France; and his relations with Diego Rivera and Frida Kahlo in Mexico. Even many of these accounts, however, (especially those devoted to Trotsky's stay in France) are often little more than elaborations of the information provided by Trotsky himself in his *Diary in Exile*.

Although the author obviously views this work as an important corrective to the published writings on Trotsky (see, for example, his syllabus of others' errors in the appendix), in the end it is a work too concerned with the trivialities of Trotsky's years in exile, and, despite Van Heijenoort's earlier proximity to Trotsky, brings us no closer to an understanding of the man, his ideas, or his importance.

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JOHN J. STEPHAN. *The Russian Fascists: Tragedy and Farce in Exile, 1925-1945*. New York: Harper and Row. 1978. Pp. xxii, 450. \$15.00.

Ernst Nolte called the period between the two world wars the "Age of Fascism," a designation based on the ubiquitous nature of the ideology. Every European country had its fascist movement, although Russia's movement, given the totalitarian nature of the Soviet state, was centered in the emigrants who had fled the country following the revolution and civil war. John J. Stephan gives a vivid portrait of the Russian emigrants: scattered around the world, homesick, rejected by their host nations, poverty-stricken, agonizing over the bankruptcy of their values, and deprived of the opportunity to live normal lives. Suffering in intensified form the anxieties that beset all the peoples of the world after World War I, the émigrés were highly receptive to fascism.

The Russian fascist movement attracted more

than the usual fascist quota of neurotics and misfits. The leader of the Manchurian branch of the party, K. V. Rodzaevsky, was a typical crackpot: he welcomed the German victories in 1941, because he expected that Hitler, as a fellow fascist, would summon him to head a restored "national Russia." When these hopes collapsed with the end of the war, he decided that Stalin had all along been a cryptofascist and had achieved Rodzaevsky's goals. Rodzaevsky accordingly turned himself over to Soviet authorities, expecting that Stalin would wish to make use of his "political talents." He was instead whisked off to Moscow to face interrogation and a firing squad.

Rodzaevsky's comrade, A. A. Vonsiatsky, the head of the American branch, was equally adept at retreating into fantasy. For example, not in a position to order the building of a real Russian fleet, he bought hundreds of model shipbuilding kits with which he proceeded to construct a Russian "navy." Employing his wife's fortune (he was married to a wealthy American heiress many years his elder) and making use of his histrionic skills, he cultivated a following among American journalists who reported his assertions that he commanded legions of fascists both inside and outside of Russia. Congressman Dickstein of the Un-American Activities Committee took it all seriously and complained on the floor of the House that the governor of Connecticut was allowing fifty thousand armed fascists to roam his state. The American government took action, and Vonsiatsky sat out World War II in prison. His bizarre career—which included service as a young officer in the elite Black Hussar regiment of the Imperial Russian Army, penury as a stagehand for the Folies-Bergères in Paris, the life of golf and fast cars of a New England squire, Führer of the world Russian fascist movement, disgrace and imprisonment as an undesirable alien, and, finally, comfortable old age in Florida—would be hardly credible, except for Stephan's careful documentation.

Stephan has researched his topic thoroughly. His command of anecdote both entertains the reader and illuminates the issue. Memorable is his portrait of Alexander Kerensky, Grand Duke Alexander, and Boris Savinkov (the SR terrorist) finding themselves seated at separate tables in a Paris cafe. They "glared at each other in speechless hostility," each blaming the others for his own fate. The author perhaps spends too much space on the factional quarrels within the Russian fascist movement and on the rogueries and sexual peccadilloes of Vonsiatsky. Most of these matters are, after all, eminently forgettable, but the shortcoming is mitigated by Stephan's considerable narrative skills. The Russian fascists never exercised power, nor did they have the slightest chance

of doing so. But an understanding of their movement throws light on the psychology of fascism in general, and in this sense Stephan has made an important contribution.

ROLAND V. LAYTON, JR.
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CAROLLE J. CARTER. *The Shamrock and the Swastika: German Espionage in Ireland in World War II*. Palo Alto, Calif.: Pacific Books. 1977. Pp. 287. \$12.95.

One might think that there would be a good deal of informed historical literature concerning the policy of neutral powers in the Second World War. But this is not always the case. Sweden and Spain are not badly off in this respect, yet Switzerland, Turkey, and some others still represent unexplored territory. Ireland belongs to this category. There may be special reasons for this situation; the main one is that Irish foreign policy is still very much connected with domestic affairs. Furthermore, Irish diplomatic papers have only recently been thrown open to the prying eyes of historians. What we know about Irish foreign policy is essentially drawn from non-Irish sources: German, British, and, above all, U.S. It is only through these channels that the Irish historian is able to reconstruct Irish policy in what were quite eventful and important years.

Carolle J. Carter's book fills a gap, and for this reason it is to be welcomed. It gives a fairly comprehensive account of the activities of German spies during the war. This was not a great and significant episode in itself, and the story requires a certain degree of refinement and subtlety, which Carter provides. She does not overassess the importance of her contribution but shows a critical faculty in assessing the numerous oral sources from which she derived her information. She is fairly well acquainted with the IRA and its manifold divisions in Irish affairs between 1938 and 1945. She relies for her purposes on captured German papers and word-of-mouth information given by "well-informed sources" in the Irish Army and police intelligence. Despite these limitations, her two principal informants, the head librarian of the National Library of Ireland, Dick Hayes, and Colonel Dan Bryan, chief of military intelligence during the war, were both extremely balanced and perceptive observers. On this account Carter emerges from some very troubled waters with flying colors.

She is mainly concerned with the activities of German agents and those of the IRA. Yet there is still some information to be derived from British and American intelligence during that period. This is not to deny the book's utility, however, at

the present stage of historical analysis. Carter undoubtedly will look forward to Robert Fisk's forthcoming book, *The Strange Ally*, which deals, *inter alia*, with British intelligence and information on Ireland in the same period.

The material on "agents" like Weber-Drohl, Reilly, Ryan, Haller, Clissman, and Veeseymayer, is interesting; their very existence and their aims throw some light on Irish and German relations during the period. The IRA in recent Irish history is always a matter of some controversy but there is little new here. Still, the episodes on clumsy organizations, such as the German Abwehr, no less than the IRA staff, not only throw light on Irish-German relations in the period, but also they raise questions as to the efficiency of German intelligence and the revolutionary nationalist organization itself. De Valera emerges creditably, though he appears very little on the "intelligence" side. Fred Boland, Joe Walsh, and Con Cremin (the latter is still active as an Irish diplomat attached to Conferences on the Law of the Sea) appear to have acted with wisdom, humanity, cunning, and discretion. Carter has had a chance to offer a general picture of Irish neutrality throughout the war; she has not done too badly. Her account of the prisoner of war camp, Friesach, is amusing and illuminating. She should have said much more about Veeseymayer (who incidentally died last December). She is good on Herman Goertz, a lonely, "semiheroic," and yet comical figure of German intelligence in Ireland between May 1940 and December 1941. His mission was perhaps more serious than Carter indicates; it was, after all, taken seriously by no less a figure than General Jodl.

In conclusion, Carter is fair to Germans and everybody else; she also shows good understanding of the problems confronting De Valera in his problem of maintaining a neutral posture. Unfortunately, there are a few gross slips. To assume that Sean MacBride was minister for foreign affairs in a Fianna Fail government is almost an unpardonable error. There are some other errata of this kind. In view of the other virtues of the book, however, and of the limited access to source material, Carter's work can be safely recommended to specialists and general readers.

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VELJKO MIĆUNOVIĆ. *Moskovske godine 1956/1958* [Moscow Years 1956/1958]. (Monografije-Biografije-Dokumenti, number 6.) Zagreb: Sveučilišna naklada Liber. 1977. Pp. 539.

There are four books published within the last two years that enrich our understanding of internal

and external policies of Yugoslavia. Dennison Rusinow's *The Yugoslav Experiment, 1948-1974* is a superbly written historical and sociopolitical chronology of postwar Yugoslavia. Dusko Doder's *The Yugoslavs* is an accurate, in-depth study of everyday Yugoslav life in the 1970s, when he was Belgrade correspondent for the *Washington Post*. The two remaining books, Milovan Djilas's *Wartime, 1941-1945* and Veljko Mićunović's *Moskovske Godine, 1956/1958*, cover shorter periods of time but are full of priceless reflections and new information since they were written by insiders who know intimately the strengths and weaknesses of communism. And yet there is a significant difference between Mićunović's book and books written by other leading Yugoslav officials. Although Djilas gave us one of the first personal portraits of Stalin and Svetozar Vukmanović-Tempo of Khrushchev—portraits based on prolonged meetings with the Soviet leaders—Mićunović, the Yugoslav ambassador to Moscow, in 1956-58 and 1969-71, offers a more complete, intimate, and objective portrait of Khrushchev, both as a leader and as a human being. It is of interest that, unlike Vukmanović's *Memoirs*, which were edited in part in order not to offend the Russians, Mićunović's diary has been printed unabridged. One can only hope, for the sake of the historical record, that Mićunović will be allowed to publish his second volume of *Moscow Years*, which deals with the much more sophisticated and cunning behavior of Brezhnev. But whatever "historical forces" (Soviet or Yugoslav) predetermine when and if the second volume will be published, the most vivid and accurate portrait of Khrushchev is already available, albeit only in the Serbo-Croatian language. Judging by the hostile Soviet reaction to the publication of the present volume and their request for a complete ban of the book, publication of the second volume hangs in the balance and depends on the momentary state of affairs between the two countries.

Mićunović's diplomatic logbook is not merely an insightful series of accounts describing official and unofficial meetings with the alternately surly or jovial Khrushchev. It is also an accurate record of fluctuating Soviet-Yugoslav relations. At the same time, it is a fascinating diagnosis of the Soviet system, its top leadership, and the everyday life and frequent hardships that even diplomats of friendly countries encounter in Soviet Russia. Even further, it is an expert commentary on such topics as the ups and downs of Cold War and detente, international crises such as Hungary and Suez, Soviet hegemonistic maneuvering within the international communist movement, and the internal Soviet crises of de-Stalinization and the Malenkov-Molotov purges. Among the Soviet successes that Mićunović notes, the most significant is the

attempt to reject Stalin's excesses, to increase consumer satisfaction, and to bring Soviet leadership closer to the people. Khrushchev is praised for becoming the first populist Soviet leader, mildly criticized for exaggerating Soviet military power after the first successful launching of sputnik, and reprimanded for the unfair firing of Marshal Zhukov. The meteoric rise and fall of Zhukov under Khrushchev (pp. 357-62) serves as a reminder that the closer one gets to the top and the more one obliges the ruler, the more dangerous he becomes to the "uncrowned communist tsar." Mićunović notes that the USSR had three coups d'état following Stalin's death: the execution of Beria and the purge of the KGB, the Malenkov-Molotov or anti-party group ouster, and the firing of Zhukov. The fourth purge of post-Stalin Russia, that of Khrushchev himself, ended peacefully as Khrushchev became the victim of his own technique of disposal by democratic vote of the Central Committee. The transfer of power from one man's whim to the larger forum is one of Khrushchev's most important contributions toward greater stabilization of political change in the USSR.

Indirectly, Mićunović confirms what this reviewer believes to be the crucial dilemma of any totalitarian system, including Soviet communism—namely, the nontolerance of any type of dissent. "Beware!" Shepilov warned Soviet writers in response to the new Chinese party line, "Let one hundred flowers bloom." "Only one weed can destroy even the most beautiful bouquet of flowers," he said. (p. 281). Mićunović also reports on the first serious Sino-Soviet confrontation during Mao Tse-tung's Moscow speech of May 17, 1957, when Mao referred to the Malenkov-Molotov and Khrushchev groups as "two factions" of the CPSU. At that moment Mikoyan stood up and scowled at Mao, then turned his back on the speaker and stared out the window (pp. 373-77). This and many similar incidents betraying Moscow's "hidden or open" hostility for socialist countries that oppose its imperial policies depict the aggressive global intentions of the Soviet leadership, which still believes and works to fulfill Khrushchev's dictum, "Today rockets decide everything" (p. 507). After witnessing Khrushchev's angry outburst at Polish leaders for "turning their backs on the USSR, looking to the West . . . and wanting to separate Poland from the fraternal commonwealth," Mićunović observes "that when Khrushchev talked about Poland he discussed it as if it were the internal problem of the USSR" (p. 62). The same could be said about Hungary.

Russian attempts to split the West first aim at alienating Western Europe from the United States, then at splitting France, Germany, and England.

To undermine European faith in America "the Soviet side constantly spreads rumors about the secret correspondence of the USSR with America" (p. 425). Khrushchev was also aware that by acting resolutely in Eastern Europe "the policy of peaceful coexistence would not suffer." "On the contrary," said Khrushchev, "imperialism accepts peaceful coexistence only if socialism is strong. Imperialism will not coexist with a weak socialism" (p. 107). To prove that point, Khrushchev crushed the Hungarian Revolution. Mićunović's notes about Soviet duplicity and treachery in this tragic event should be published in English. They include a detailed account of a secret trip that Khrushchev and Malenkov made to Brioni Island on November 2, 1956 to gain Tito's approval for squashing the Nagy government. It was a ten-hour, eye-to-eye meeting at which Mićunović was present. After Khrushchev's departure at 5 A. M. on November 3, the orders were given to attack Budapest, and by November 4 Nagy was already seeking asylum in the Yugoslav embassy. It is well known how the Russian and new Hungarian governments betrayed their pledges to Yugoslavia and destroyed Nagy. What has not been fully recognized is the complex neo-Stalinist legacy that predetermined the aggressive foreign policy. This is presented graphically and lucidly by Mićunović. His book is a historic document that must be read.

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A. E. MUSSON. *The Growth of British Industry*. New York: Holmes and Meier. 1978. Pp. 396. \$29.50.

A. E. Musson's history of British industry falls into four parts: a short discussion of the preindustrial economy; a second part consisting of five chapters covering the industrial revolution, which is dated from 1700 to 1850; a main section of seven chapters on the period from 1850 to 1914, entitled "Mass Production and Mass Consumption"; and a solid final part of five chapters, "Decline and Growth," covering 1914-39. The history does not cover the period after the Second World War beyond a few forward references.

As one would expect from his earlier work, Musson focuses on technical change. He covers the main industries—textiles, coal, iron and steel, engineering, chemicals, and electric—and a host of other usually neglected trades such as clothing, leather, coaches, watches, and clocks. Tables abound, especially on the first census of manufactures in 1851, with information on employment, power capacity, exports, and the like. There is little scholarly apparatus, such as footnotes, but an

excellent bibliography and frequent ascription of views to individual historians; the source is generally clear from the bibliography.

The point of view is a little confusing. Musson does not share a number of widely held views on British industry—on take-off or a spurt, for example, in the period from 1766 to 1775, on the Great Depression, or on the Climacteric. He frequently characterizes the views of this sort as “exaggerated,” “over-emphasized,” or “misleading.” Without using econometric methods, he tends to share the attitude of the revisionists, such as McCloskey, Temin, Sandberg, and others, plus the noneconometrician Saul. Musson opposes the viewpoints of Burn, Burnham and Hoskins, Landes, and others with regard to iron and steel and engineering. He quotes McCloskey approvingly with regard to entrepreneurs in iron and steel, who “did very well indeed” (p. 176); he notes that American techniques in engineering were “quickly” or “soon” adopted (p. 187) and that British firms were “quick to grasp” their opportunities (p. 189). At the same time, Musson’s pages are filled with descriptions of one or another industry that lagged behind the corresponding one in Germany and the United States. The evidence seems to point in a direction different from the stated conclusion.

Musson virtually neglects the impact of entrepreneurial sociology, and he attributes less weight to industrial organization than this reviewer thinks appropriate. Chapter fourteen, on “Industrial Structure and Organization,” deals very lightly, if at all, with such questions as the inability of the market to cope with technical progress, which requires related investments in separate corporations, or with the merchant as an inhibitor of technical change in such industries as machine tools. Musson mentions the fact that the shipbuilder was an assembler of components manufactured by different suppliers, but he does not evaluate its efficiency in a given technological state or note its possible inhibition of technical change. He also neglects the role of the second-hand market in ships that for so long accelerated new building in Britain.

Although one may thus indulge in slight differences with Musson with regard to interpretations, the profession is solidly in his debt for the well-organized account of British industry from 1700 to 1939.

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JOYCE BELLAMY and JOHN SAVILLE, editors. *The Dictionary of Labour Biography*. Volume 4. New York: Augustus Kelley. 1977.

This is the fourth volume of a continuing series whose editors have set themselves the stupendous task of producing a multivolume biographical dictionary of all the important figures in the British labor movement since 1790 (excluding living persons). The idea originated with the late G. D. H. Cole and was taken up by the editors with the assistance of the Society for the Study of Labour History. The intention is excellent: to provide a complete coverage not only of national figures but also of those vitally important men and women who operated only at the local level. But the size of the undertaking is enormous and has been absolutely reliant on the help of a very wide range of librarians, archivists, trade unions, members of the society, and so on; it is meant to be a co-operative venture. So far over eighty people have contributed, but in fact a very large proportion of the entries that have so far appeared are written wholly or partly by one of the editors or their team of research assistants, who now number three.

The potential value of such a work is, of course, very great. It provides full and accurate biographies of well-known labor figures and, equally if not more importantly, many on lesser and even virtually unknown figures who are thereby rescued from obscurity. The value of the entries is greatly increased by the full lists of sources given at the end of each entry, much longer than is normal in biographical dictionaries. The work is therefore a research aid of the first order; to some of the entries are appended subject bibliographies, on such topics as co-operation and mining trade unionism in different periods, Christian socialism, new model unionism, and the early Labour Party. But the number of these subject bibliographies has fallen steadily, unfortunately, from eighteen in the first volume, to four in the second, two in the third, and only one in the fourth.

The size of the task meant that a complete dictionary could not appear all at once, and volumes have appeared at intervals. Nor could the early volumes be devoted to names beginning with “A,” so each volume has a range of entries that were ready at the time, with no alphabetical concentration. An attempt has been made to avoid a wholly miscellaneous character in each volume by concentrating on certain topics. About 70 percent of the entries in the first three volumes deal with co-operation (especially in the first volume), miners, and MPs elected before 1914. In the third volume the editors include more topics, such as iron trade unionism, Christian socialism, and the early Independent Labour Party. But so far there has been very little on the Chartists or earlier periods. In view of cost considerations and the time it takes to produce a volume, it will be a very long time, if ever, before a complete dictionary appears to which one can go, confident of finding an entry on

any important figure. To help use the volumes that have appeared, the editors include a cumulative index of all entries that have appeared at the end of each volume; there are many cross references, including entries yet to appear, and, above all, each volume has a general subject index, an outstanding innovation that vastly increases the value of the dictionary. As time goes on, of course, additions to entries and bibliographies will be necessary, and future volumes will include these.

This fourth volume brings the total number of entries to over 520. There are entries on Annie Besant, the two Blatchfords, Havelock Wilson, and Ben Tillett (to which is attached a consolidated bibliography on new unionism). Altogether it has entries on twenty-one Labour MPs, sixteen miners' leaders, a few each on late nineteenth- and twentieth-century trade unionists, co-operators, socialists, Communists, and four Chartists. The volume maintains the very high standards of the preceding ones, with biographies and bibliographies based on meticulous scholarship and thorough research. The launching of the dictionary has been an important event in the study of British labor history, and we must all hope for the rapid appearance of succeeding volumes of this work of great scholarship, a mine of information and an invaluable aid to research.

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RICHARD D. ALTICK. *The Shows of London*. Cambridge: Harvard University Press. 1978. Pp. 533. \$35.00.

Within the heart of every historian lurks a pack rat, happily collecting historical tidbits for future use. This book is a treasure-trove of odd facts, most of which add to our knowledge of the history of popular entertainment. Indeed, the range of material offered here is a tribute both to Richard D. Altick's scholarship and to the British tradition of cherishing artifacts of the past. The combination has yielded a book of interest to scholars in history, folklore, theater, and popular culture. Every reader will find her or his favorite item. My own is an amusing addendum to the seventeenth-century fascination for exotic animals. A London merchant in 1636 wrote "to a factor of his beyoand [sic] sea" to send him "2 o[r] 3 apes," but left off the "r." The diligent factor replied by sending four score and promising the remaining 123 by the next ship (p. 37). The price of apes dropped temporarily on the London market!

Altick's book is a fitting sequel to his *English Common Reader* (1957). In the latter he documented the growth of a popular literary tradition and early efforts to promote cheap reading matter of a non-

religious sort. In *The Shows of London* he explores the other alternatives for the spare pennies and limited leisure of the working and middle classes. For those who could not read or would not, the enormous variety of shows—from animal freaks to mechanical novelties, with plenty of human oddities along the way—satisfied curiosity and intellect. Altick is at his best when showing the sheer quantity of shows available through many decades. He traces, for example, the development of mechanical shows demonstrating the latest inventions, "monster-mongers" who remained perennially popular, the popularization of art, and the rise of large-scale scenarios, variously called panoramas, dioramas, "eudophusikons," ("representations of nature" with three-dimensional effects), and other novel coinages. Although no color plates are included, the book is exceptionally well produced and well provided with excellent illustrations. Those that accompany the description of the building of the London Colosseum are particularly interesting. Built in the 1820s, the Colosseum was designed to combine an enormous panorama of London (a circle of canvas 134 feet in diameter and sixty-four feet high, totaling some 46,000 square feet was painted) with the added features of music, art, café, Swiss cottage, water falls, and so forth.

For the historian, Altick's most valuable contribution is his painstaking reconstruction of the costs, admission charges, and profits (or quite often losses) of these shows. The Colosseum, for example, never consistently realized a profit and was auctioned off in 1843 at 23,000 guineas—less than half the original cost. It reopened, freshly remodeled with the latest mechanical gadgetry, two years later, only to be auctioned again in 1855. This time it failed to reach its reserve price of £20,000. Temporarily salvaged and reopened as a variety house, it closed permanently in 1863. The history of the Colosseum—one of the most grandiose of London's shows—indicates both the enormous amount of money entrepreneurs were willing to invest in show business and its continually risky nature. Huge profits could be made, as the long-established Madame Tussaud's waxworks proved, but it was essential to combine familiarity of name and product with novelty. The continually changing waxwork figures, along with the popular Chamber of Horrors, plus the claims to authenticity—Mme. Tussaud's standards of reproduction were much higher than those of her rivals—could mean success despite fickle public taste.

A book as various as this will offer something for everyone, but I found the most interesting section to be that on the development of free public museums, a process that took many years and was not fully completed until this century. Altick is scrupulously fair in describing the fears felt by the gov-

ernment bureaucrats and middle-class public about offering free museums, or even museums open at hours when the general public was not normally at work. The drunkenness and disorderly behavior of crowds on holidays and at cheap shows was notorious. It was widely believed that free museums would become resorts for down-and-outers anxious to get out of the rain and cold or that drunks—and perhaps simply the uneducated—would deface the paintings and objects on display. Experience had shown that when Westminster Abbey and St. Paul's dropped or lowered their charges for viewing, many found their nooks and crannies convenient as urinals or for assignations, and other unsavory purposes. But a more generous public spirit prevailed, and despite the continuing parsimony of the government, the British came to offer free cultural centers with liberal hours, in contrast to the rest of Europe.

Unfortunately, at times Altick lets the material take over from his analysis of it, so that the reader is left with a sense of too many facts accumulated to no very clear end. Many more linkages could have been made with the changing historical conditions that leisure-time activities so often reflect. The wholesale pursuit of "realism" in art and shows from the eighteenth century could be profitably explored as could the changing attitudes toward "the Savage." But the richness of the material itself never palls, and we are all indebted to Altick for displaying it—to use a showman's term—for our benefit. He has given us a most varied history that will surely have a more profitable and longer-lasting career than the Colosseum.

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GORDON K. LEWIS. *Slavery, Imperialism, and Freedom: Studies in English Radical Thought*. New York: Monthly Review Press. 1978. Pp. 346. \$15.00.

Eight essays originally published in academic journals over the past decades—where and when they were published we are not told—have been revised by their author to provide an exploration of English radical thought from the eighteenth century to the present. The chapters concern the debate over slavery that preceded British abolition, Bulwer-Lytton as an aristocrat with a social conscience, three Victorian free thinkers (Francis Newman, Mark Pattison, and George Elliot), the Christian socialists, the Paris Commune of 1871, the Fabians, Winston Churchill as historian, and race and color in contemporary Britain. For each essay a bibliography at the end of the volume offers suggestions for further reading, but there is no index or formal citation of the quotations and ref-

erences in the text. It is not clear for what audience the author intended the book, but it will be interesting only to readers already acquainted with the topics under discussion.

Gordon K. Lewis is a political scientist writing intellectual history. A committed socialist/Marxist, he focuses primarily on the radical thought of the English ruling classes and the extent to which it dealt with human welfare. He traces, as it were, the social conscience of the capitalist class. In Burke's attack on Warren Hastings, for example, he finds eighteenth-century cosmopolitanism and belief in universal humanity giving rise to the concept of imperial trusteeship. In the last essay of the book he returns to the theme of empire and sees in the racial issues of West and East Indian immigration the legacy of nineteenth-century ethnocentrism and authoritarianism. It is a legacy that the intervening essays trace.

The chapters on the Christian socialists and the Fabians are particularly interesting for the method by which he peels back, layer by layer, the dominant and discordant intellectual currents in each movement. If there is a striking note that runs through all the essays, it is the role nostalgia plays in English radical thought, whether it is nostalgia for the ideal of a simpler, more egalitarian past or a nostalgia for the ideal of a simple, rational human being, for whom efficient civil service will provide the ultimate answers. Lewis interweaves the goals and paradoxes of these movements in a rather elegant way. He is struck by the failure of the Fabians to concern themselves with a "satisfactory theory of the state" (p. 251), and, if anything, he does not stress enough how thoroughly authoritarian their intellectual pretensions were in arranging life for lesser minds as well as lesser breeds.

The pithiest chapter of all is that on Winston Churchill as the historian of the "master class." The embodiment of "English pride and prejudice" (p. 269), Churchill has set forth in his many books a series of self-justifications: for politicians against soldiers, for his own national leadership and that of his family forebears, and for all the great men of action who strode the stage of English history with the manners of gentlemen. The English "people," in contrast, are the "faceless audience in the theater of history" (p. 279). There is much to disagree with in the book, which is to say it is both provocative and, for the most part, interesting to read.

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RANDOLPH TRUMBACH. *The rise of the Egalitarian Family: Aristocratic Kinship and Domestic Relations in*

Eighteenth-Century England. (Studies in Social Discontinuity.) New York: Academic Press. 1978. Pp. xviii, 324. \$17.50.

Randolph Trumbach sets out to discover how between 1690 and 1780 three generations of the titular English aristocracy changed their attitudes toward the four circles of relationships that enveloped them: their wives and children; their household servants; their kindred; and the friends, allies, and clients who made up their patronage "connexion." His principal sources are the family correspondence, marriage settlements, and wills of some forty of the one-hundred-sixty aristocratic families of this time, together with scattered materials for some twenty more. Trumbach borrows his central organizing ideas from the social sciences. He takes the distinction between patrilinear and kinship principles from the anthropologist Robert Fox and adopts John Bowlby's theory of "attachment" as the principal infantile need. He firmly rejects both Freudian and Eriksonian ideas about the psychology of the child.

Armed with this theoretical apparatus and these data, he shows that by 1750 the aristocratic family had been transformed. There was now equality between man and wife, permitting marital friendship. Marriages were now romantic and not arranged. Wives and even husbands spent far more time with their children. Mothers breast-fed their infants, abandoned swaddling, and gave their children constant loving care. And lastly, the family group detached itself from the servants who so closely enveloped them.

It is evident that, working totally independently—for neither of us saw each other's manuscript—Trumbach and I have arrived at very similar conclusions about the aristocratic family. This coincidence suggests that our findings must have some objective validity, especially since we used substantially different source materials. Trumbach combed the archives for marriage settlements and also found a good deal of unpublished correspondence and diaries, whereas I relied almost entirely on printed sources. As a result his most important new contribution is contained in his chapter on "Settlement and Marriage," in which he takes the model of the strict settlement provided twenty-eight years ago by Sir John Habakkuk and refines and develops it in hitherto unknown ways.

The weaknesses of the book are twofold. In the first place, although Trumbach has studied no other class but the aristocracy, he claims that "it was the aristocracy which led the way" (p. 289), an assertion that is very difficult to defend if one looks at the upper bourgeoisie and the squirearchy. Furthermore, Trumbach makes virtually no effort to explain why so remarkable a change in basic familial attitudes took place when and where it did.

He only devotes one page to this question, contenting himself with a brief statement that "the fusion of these four factors—commercialization, religion, political control and kindred structure—may have produced that partial egalitarianism in familial relations that we have called domesticity" (p. 122).

These criticisms, however, should not obscure Trumbach's success in digging out the materials and analyzing the facts of so momentous a change in the history of the aristocratic family. He concludes this useful monograph with the intriguing suggestion that the establishment of partial egalitarianism within the late eighteenth-century aristocratic family may have helped the class to accept the early nineteenth-century change to a more egalitarian political order and so stave off the threat of revolution.

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W. A. SPECK. *Stability and Strife: England, 1714-1760*. (New History of England.) Cambridge: Harvard University Press. 1977. Pp. 311. \$15.00.

In this contribution to "The New History of England" W. A. Speck has not been content simply to produce a narrative account of high politics in the reigns of the first two Georges. Rather, he offers a theme and an argument: that the creation of political stability in this period rested on the emergence of a unified governing class and that this, in turn, has to be explained by social and economic as well as political considerations. His central point is that the party divisions that dominated Anne's reign arose from economic and social tensions as well as from more immediate disagreements over matters of policy. Political stability became possible after 1714, in his view, not only because the issues disappeared, but because these tensions in society also gradually abated and allowed the formation of a governing class united in its admiration of the constitution. To present this argument, Speck devotes more than half of the book to general chapters on the social and economic structure of England and its political institutions in the first half of the eighteenth century, and only then does he follow with a narrative account of politics at court and in parliament. A chapter on "The Making of the English Ruling Class," in which Speck asserts his argument, provides the link between the structural part one and the narrative part two.

The book is boldly conceived and the result has much to recommend it. Speck writes well and with a fine eye for the illuminating detail, and the general chapters in particular will provide students with good introductions to a number of topics. He is at his best in his sketch of political institutions,

especially parliament and the electoral system, but he also writes well about the churches and Methodism. The two chapters on social structure and social change, on the other hand, are rather more diffuse and are limited inevitably by the state of research and the poverty of our knowledge in so many areas.

Students and the "general reader," whom the publisher is also hoping to interest, will find much of value in these thematic chapters. But it is perhaps a consequence of the decision to devote so much space to them that the narrative in part two is sometimes squeezed more than is comfortable. Although the book is never unclear, the discussion of foreign policy sometimes comes across at something of a gallop, and the account of domestic politics, too, is occasionally compressed more than Speck must have wished it to be. It is also a consequence of the way the book is organized that the argument, which is convincing in general terms, remains asserted as an introduction to the narrative rather than shaping it and growing from it. And in one respect Speck needs to state the argument with a little more flexibility, for it is surely necessary to take into account the growing numbers of men, especially in London, who were never fully brought within a unified ruling class—or who were brought in only temporarily by Pitt—and who within a few years of the close of this period would be supporting Wilkes and drawing inspiration from America in a campaign to reform Parliament.

Speck set himself an ambitious task, and he is to be congratulated for not contriving yet another general account of the rise of Walpole, the difficulties of the Pelhams, and the triumphs of the Newcastle-Pitt administration. His book is welcome, if only for the encouragement it will give to students to look beyond the House of Commons for explanations of the changing character of politics in the first half of the eighteenth century.

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PETER RAZZELL. *The Conquest of Smallpox: The Impact of Inoculation on Smallpox Mortality in Eighteenth Century Britain*. Lewes, Sussex: Caliban Books. 1977. Pp. x, 190. £8.00.

Peter Razzell has written an interesting and argumentative book setting forth the general thesis that smallpox inoculation (or variolation, as distinct from smallpox vaccination) was widely accepted and employed in England during the latter part of the eighteenth century and that it led to a significant saving of lives. Razzell describes the change in inoculation techniques from a relatively deep incision used after the practice was introduced in

1721 to the very slight incision employed by the Suttons beginning in the 1750s. He credits this change, which became widespread during the 1760s, with reducing the severity of inoculated smallpox to a relatively mild affliction in most instances and a very low case fatality rate. He also argues that the Suttonian technique further decreased the likelihood that someone with inoculated smallpox would convey the infection through the natural respiratory route to susceptible persons; as a result, the danger of natural epidemics arising from inoculated cases became negligible. The Suttonian inoculation also made the process cheaper, especially as preparatory treatments were given up; the technique became available to the middling sort and even to the poor, either by physicians' charity or payment by the overseers of the poor. The latter, in particular, favored inoculation when it became cheaper than caring for and burying the victims of natural infection. General inoculations, when virtually all susceptible persons were treated within a few days, became increasingly common, first in the countryside and villages and later in the towns (but never in London). All of this led to a considerable decrease in smallpox mortality after the 1760s.

The evidence presented by Razzell is taken largely from an extensive investigation of contemporary accounts of smallpox and inoculation by medical authors, supplemented by local histories and other records. This reviewer, who has previously argued in a more limited setting that inoculation could and did reduce smallpox mortality, believes that Razzell has demonstrated in a convincing manner the increasing use of, and decreasing case fatality rate from, inoculation and that his argument in favor of the Suttonian technique is well supported. He has carried the examination of the history of inoculation beyond that of previous medical historians and has contributed substantial support to the argument that this medical procedure could significantly affect the general death rate. As nearly as he can calculate, about 15 percent of those born died of smallpox in the eighteenth century before inoculation was introduced. It is not possible to give convincing statistics for England as a whole to show the extent of the mortality reduction, but anecdotal evidence is enough to indicate that the result was substantial and, had the technique been used universally, it might well have lowered smallpox mortality to a demographically negligible figure.

The reasons proposed by the author for the success of inoculation are admittedly speculative and, from a scientific standpoint, we may never know the answer. Less convincing are Razzell's conclusions that without inoculation and later vaccination (which he considers virtually identical on the basis of arguments in an earlier book) one-

quarter to one-third of the population of Victorian England would have died of smallpox and the country would have stagnated rather than experiencing a booming Industrial Revolution. Nevertheless, for the period up to 1798, it is a valuable contribution to the history of medicine and its effect on the population and a refreshing contrast to the current tendency to find nothing good in medicine before 1900.

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A. D. HARVEY. *Britain in the Early Nineteenth Century*. New York: St. Martin's Press. 1978. Pp. 395. \$22.50.

This book is divided into three sections. The first is a brief social history of Britain at the turn of the nineteenth century, which touches on religion, the structure of society, and social unrest. The second section is essentially a political narrative extending from the apex of Pitt's power in 1800 to the more troubled times of Perceval's assassination in 1812. Then follows a short concluding section, mainly concerned with economic policy and the economic impact on Britain of the war with France. Such a comprehensive treatment ought to be welcomed. But unhappily, these sections are not well integrated (as the author himself engagingly admits on page 5), and the book gives the distinct impression that A. D. Harvey has fallen between several stools.

Of the three sections, Harvey seems most at home in the political, which at two-thirds of its length is the heart of the book. He presents matters of high politics clearly and straightforwardly, if without surprises. One might well look here for a competent rendering of the important domestic and diplomatic events of 1800-12. We are also led evenhandedly through the intrigues of office among Foxites, Grenvillites, Pittites, and other aspirants for position. Readers may only be disappointed that Harvey did not say more about the issue of the royal prerogative, which Harvey himself calls the "real key" to political conflict of the time.

When Harvey moves away from court and cabinet, he treads less surely. His statements about social categories are overly generalized and lack sophistication. He claims, for example, that England's rulers, an "oligarchic caste," had increased their hold on social institutions by 1800; but the evidence cited is little more than literary and anecdotal. His references to the "middle class" lack precision. The reader learns that the middle class was startled, confused, and eventually alienated by the scandals of the age, but one is never sure who the middle class might be. The same reservation

can be held about "lower-class" aspirations and political activity. Refinements in the use of these terms are crucial to an understanding of the growth of reform sentiment—a topic of importance during this period, as Harvey rightly reminds us in the latter stages of his book. Indeed, Harvey believes that Britain was near revolution in 1809-12 precisely because of the disillusionment of the lower- and middle-class members of society. Until the various strands of the reform movement are unraveled, however, one is unlikely to learn how reform sentiment worked its way through society. This is an immense topic, still awaiting its historian, and Harvey cannot be held strictly accountable for failing to explain it more successfully than he has.

One final note might be entered here. Someone should have looked more carefully at the proofs. Typographical errors include: "Corbett" for Cob-bett (p. 8); "superios" for superiors (p. 56); "secession" for succession (p. 205); "Castlgh" for Castlereagh (p. 258); and "ultimated" for ultimately (p. 307). There are lines omitted on pages 39 and 74; and a line is repeated on page 99.

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ANTHONY BRUNDAGE. *The Making of the New Poor Law: The Politics of Inquiry, Enactment, and Implementation, 1832-1839*. New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xv, 204. \$12.50.

The New Poor Law of 1834 established a new system of social welfare in England that lasted until the passage of the National Insurance Act of 1911. The new system was designed to deter poverty by enclosing paupers in workhouses, where conditions were made less attractive than the living standards of the poorest paid laborer in private employment. Upon entering the workhouse, indigent families were separated, in keeping with Benthamite principles, husband from wife, and children from their parents. Anthony Brundage leaves to others these welfare aspects of the New Poor Law in order to concentrate on the problems of its administration. His chief desire is to participate in the academic debate over "the revolution in government," and he is eager to show that the extent of centralized authority in the administration of the New Poor Law was not as great as some historians previously have supposed. The New Poor Law created a new system of administration, uniting 15,000 parishes into poor law unions. Brundage argues that the new system, rather than greatly increasing the power of the central government, actually created permanent and autonomous units of local government under the domination of the landed interests.

His main contribution is a detailed study of the implementation of the New Poor Law in ten selected counties of the midlands and East Anglia. Using the correspondence of many assistant commissioners with the central board, the author documents the problems of forming the poor law unions in keeping with the interests of the large landowners; but he sometimes fails to distinguish between great Whig aristocrats and the lesser Tory gentlemen—the latter were the beneficiaries of the old law and the former of the new. Rarely referring to the justices of the peace, the Tory administrators of the old law, the author prefers the more general term, “magistrates,” and in so doing he blurs the distinction between the paid officials of the urban parishes and the county justices of the peace. The author’s selection of ten rural counties for study leaves untouched the more complex problems of the new urban parishes of London and the industrial regions of the North. He fails to deal with the main problem that the burden of the rates fell on the occupier of land and not on the owner, the tenant farmers who hired the laborers and provided them with relief in seasonal unemployment. To bridge the limitations of this book, the reader should turn to N. C. Edsall’s *The Anti-Poor Law Movement, 1834-44* (1971).

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ANTHONY S. WOHL, editor. *The Victorian Family: Structure and Stresses*. New York: St. Martin’s Press. 1978. Pp. 224. \$16.95.

Until recently, family history has been synonymous with demographic history. But S. G. Checkland’s study of the Gladstones, Bruce Mazlish’s work on James and John Stuart Mill, and a stream of books and articles about domestic life and the home all suggest that the historical study of the family is taking new directions.

The collection of essays reviewed here came from a symposium on the family held by the Northeastern Victorian Studies Association in the spring of 1975. The volume begins with a useful survey of recent work in family history by the editor, Anthony S. Wohl. The remaining nine essays have been divided into two groups. The first group explores the internal workings of the family, while the second treats the family in relation to the economy and society. Of all the essays only R. Burr-Litchfield’s illuminating study of family work patterns and fertility in Stockport takes a demographic approach. The other essays depart from demography to take up such topics as childbirth, incest, the Victorian paterfamilias, and the family as an agency of social mobility. Two quite different treatments of Victorian fiction and the

family appear in the essay on “sensation fiction” in the 1860s and in the study of the relation between the Brontë family’s crises and the writing of *Jane Eyre*. No single class or group dominates the essays. The subjects range from the working classes to Queen Victoria, from nannies to writers, businessmen, and reformers. Some might fault the volume for its lack of coherence; others will praise it for demonstrating the diversity of approaches available to historians of the family.

Few of our stereotypes of the Victorian family survive these essayists’ scrutiny. John Hawkins Miller’s study of childbirth explores the “rituals and customs of the lying-in chamber.” While labeling the Victorians “prudish” with regard to physiological topics, he demonstrates that Victoria herself was not only an experienced and devoted mother but a remarkably earthy and matter-of-fact woman when writing privately of married life and maternity. Similarly, Theresa McBride takes some of the gloss from our image of middle-class family life by showing that the typical nanny was not the rosy, rotund figure of aristocratic memoirs but a young and inexperienced girl, often naive, sometimes even crude.

Elaine Showalter, exploring the popular “sensation fiction” of the 1860s, uncovers bigamy, adultery, murder, and youthful rebellion in the fictional Victorian family. She argues that the popularity of such fiction reveals profound ambivalence and hidden rebelliousness among the well-behaved young ladies who read them.

David Roberts seems, on the other hand, to confirm our stereotypes of the Victorian paterfamilias. After surveying 168 Victorian memoirs, he draws the conclusion that ruling-class fathers were remote, sovereign, and benevolent—not a startling departure from the common image. But his careful reading of the memoirs gives substance and complexity to these categories. And, by speculating on the effects of such fathers on the personalities of their sons, Roberts attempts to tie family history to the larger issues of class structure and empire.

The essays have their shortcomings. Some rely heavily on social science language, without drawing any clear benefits from the exercise. Some, like Wohl’s concluding essay on working-class incest, are too brief to offer more than a glimpse of a fascinating subject. Michael Brooks’s essay on the Ruskins and Maurianne Adam’s study of the Brontës are, perhaps, too purely literary to have extensive usefulness for the historian.

Scholars have much to learn about the Victorian family. The essays published in this volume present a useful, sometimes stimulating, vision of the paths open to us.

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J. DEREK HOLMES. *More Roman than Rome: English Catholicism in the Nineteenth Century*. Shepherdstown, W. Va.: Patmos Press. 1978. Pp. 278. \$16.95.

Neither the first nor even the most fanciful assertion of its kind, Cardinal Manning's "more Roman than Rome" has furnished the title for J. Derek Holmes's survey of English Catholicism in the nineteenth century. And aptly so, considering the Church's dramatic reorientation after 1850; when "more ultramontane than the Pope himself" served both to complete Manning's thought and to proclaim the end, at long last, of all detachment from the Holy See.

Well might aristocratic laymen have frowned at the prospect suddenly before them: their authority terminated in favor of Oxford converts and Irish immigrants, their history at a discount, and their assumptions overwhelmed by papal imperatives—and these, implicitly as abrasive as the penal laws or the erstwhile operation of the Vicariate. Long since tested by secular and spiritual governors alike, their latest term of trial found them emphatically English still, too firmly wedded to the ideals of 1829 to welcome a "second spring" so pungently redolent, not of medieval romance, but of the Counter Reformation. On this score, over and above the liturgies recovered from the Baroque and a belligerence worthy of Trent, "more Roman than Rome" could not but infinitely offend, whether by fostering a segregation wholly at variance with the laity's experience and aspirations or by ironically confirming virtually every imputation in the lexicon of "no popery."

What with the significance to the Church of the aristocracy's wealth, influence, and prestige, their alienation necessarily deserves the closest attention and analysis—a treatment more telling, at any rate, than that begrudged by Holmes, whose concentration upon successive prelates and primates breathes next to nothing of the lives and opinions of an embattled cousinhood, not to mention the laity at large. Take the thirteenth Duke of Norfolk, for example: did his antagonism to the Restoration generate any durable opposition to Westminster? Or did the short subscriptions, so frequently embarrassing to the hierarchy, substitute for an organized lay defense against ecclesiastical dictatorship? And what of the conflict between segregation and assimilation: to what extent did the one prevail over the other, only to force the parting of friends, the revival of odious distinctions, and a decline in the rate of intermarriage? Again, what explanation comes to mind with regard to such a figure as the third Marquess of Bute: himself a convert, but sufficiently Old English in spirit for his munificence to reflect an ecumenism quite at variance with the precept, "more Roman than Rome." In short, one not only

wonders whether ultramontaniam, in all its facets, achieved a total and unqualified triumph but also whether Holmes's focus has not distorted the realities of his subject.

The truth to tell, not even his chosen characters receive their due, not in light of his inattention, for instance, to the curious conflict between their Englishry and their obedience to Rome, between their avowedly "masculine" aggressiveness and their inveteracy for submission. If, in extenuation, Holmes aimed rather at synthesis than originality, still his frequent failure to credit his sources merits the strongest censure.

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DAVID WILTSHIRE. *The Social and Political Thought of Herbert Spencer*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. xii, 269. \$18.00.

The title of this book should not lead the reader astray: within its pages one may find not merely an exposition of Spencer's social and political theory but also a cogently argued thesis concerning the content and contradictions of the Spencerian system. In the introduction David Wiltshire puts forward his thesis with clarity and dispatch. He rejects the prevalent view that Spencer's political ideas were a function of his acceptance of evolution as the key concept for understanding both the physical and social worlds. Instead, he insists that "Spencer was an individualist liberal first and an evolutionist second" (p. 1). Yet the latter's use of evolutionary theory to buttress a politics of liberal individualism was foredoomed to failure, since liberalism and evolutionism rest on mutually incompatible assumptions about the nature of man and society.

In order to demonstrate what he describes as the "primacy of politics," Wiltshire reaches back into Spencer's family background and into that provincial culture of middle-class Nonconformist radicalism, which profoundly influenced Spencer. Subsequent chapters follow a biographical approach, thereby linking the development of Spencer's thought to his friendships, his activities as railway engineer and journalist, and the intellectual and social milieu in which he lived. Wiltshire emphasizes, for example, the fact that Spencer's first major work, *Social Statics* (1851), owed much to the ideas of Thomas Hodgskin, with whom Spencer worked on the staff of the *Economist* during the period in which he wrote *Social Statics*. Although primarily a celebration of natural rights, individualism, and *laissez faire*, *Social Statics* did evince an embryonic interest in social evolution. In the years after its publication, Spencer moved increasingly

toward evolutionary theory as a means of providing scientific justification for his social and political stance. By 1860 he accepted evolution as the grand organizing principle of both nature and society, and he spent the remainder of his life pursuing its endless ramifications in the many volumes of the *Synthetic Philosophy*. The irony of his career was that although devotion to the precepts of liberal individualism had been the starting point and remained the *raison d'être* of Spencer's theoretical endeavors, it was his evolutionary theory, frequently divorced from his politics, that brought him fame and influence.

In the second part of this book, the author shifts from a developmental to an analytical approach in order to evaluate the strengths and weaknesses of specific aspects of Spencer's social and political theory. Although these chapters are even-handed and full of nuance, their thrust is essentially critical, especially when dealing with Spencer's attitude toward state intervention and his distinction between military and industrial societies. The chapter entitled "Spencerian Liberalism and its Liberal Critics" is of particular interest to historians, since it charts the shift away from what C. B. MacPherson calls "possessive individualism"—and which Spencerian political theory exemplifies—to a variety of liberalism that accepts intervention by the state as a means of removing impediments to freedom.

But the most original and telling pages in this section, and indeed throughout the book, are those in which Wiltshire develops his central thesis. The mainspring of liberalism, he contends, is the concept of the autonomous individual, whose rights and interests it is the function of society to preserve and harmonize. Social evolution, on the other hand, because it recognizes no distinction between the processes that operate within nature and those that operate within society, reduces individuals to cells within a social organism, subject to the immutable laws governing survival. Again and again Wiltshire points out how inconsistency results from Spencer's attempt to yoke together irreconcilable ideas deriving from the postulates of liberalism and social evolutionism. His argument is compelling, and his book as a whole is to be recommended for the incisiveness of its exposition and analysis. It is a worthy complement to J. D. Y. Peel's *Herbert Spencer: the Evolution of a Sociologist* (1971), which examines Spencer's thought within the broader framework of the sociology of knowledge and the development of the discipline of sociology.

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EDITH H. WHETHAM. *The Agrarian History of England and Wales*. Volume 8, 1914–39. JOAN THIRSK, general

editor. New York: Cambridge University Press. 1978. Pp. xxiii, 353. \$29.50.

This is the third volume so far published in the projected eight-volume agrarian history of England and Wales, the first volume of which is to deal with prehistory and the last, Edith H. Whetham's book, deals with the years 1914–39. It was originally intended that this volume would end in 1964, but according to the new editor-in-chief, Joan Thirsk, it was decided to end the volume and end the series earlier; "1939," according to Thirsk, "is an appropriate concluding date for a work of history."

This editorial pronouncement is not explained, but it may be that Thirsk has in mind a kind of watershed in English agrarian history, represented by the outbreak of the Second World War. For even in the years covered by Whetham's volume, that is, 1914–39, English agriculture was still heavily dependent on horse and manpower and on long-standing techniques established in the nineteenth century. Moreover, government policy still had little effect on rural life, and agricultural output was by and large not much different than it had been in the late Victorian period. The Second World War transformed all of this; in effect, it ushered in a new agricultural revolution, the ultimate consequences of which are still not fully visible. The decision to end the projected history in 1939 would therefore seem to be a sensible one.

Unlike H. P. R. Finberg and Joan Thirsk, who edited the two published volumes in the series covering the periods A.D. 43–1042 and 1640–1750, respectively, Whetham has written the entire volume. She states at the beginning that her intention is "to provide an account of what it felt like to be a landowner, farmer, or farmworker in England and Wales in the twenty-five years from 1914–1939" (p. xxi). Unfortunately, Whetham has not succeeded. She devotes little more than a dozen pages to the rural classes, and this is not really enough to make them come to life, that is, adequately to delineate lifestyles and social structures. The author needs to do more than insert a brief quotation from Flora Thompson's novel, *Lark Rise to Candleford*. Oddly enough, F. M. L. Thompson's *English Landed Society in the Nineteenth Century* (1963), which has a twentieth-century epilogue, finds no mention in her bibliography.

Whetham also states at the outset that she has "deliberately excluded from [her] text the mathematical treatment of quantifiable factors, beloved by econometricians and counter-factual historians." She has certainly lived up to this promise; Whetham has written a somewhat old-fashioned piece of economic history. This perspective may have certain drawbacks. For example, there may be too little said in the book about such analytical

questions as the level of investment. On the other hand, being a practitioner of old-fashioned economic history has not prevented Whetham from writing a useful and intelligent book that tells us a great deal about a period in English agrarian history that has so far been neglected.

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WOLFGANG KRIEGER. *Labour Party und Weimarer Republik: Ein Beitrag zur Aussenpolitik der britischen Arbeiterbewegung zwischen Programmatik und Parteitaktik (1918-1924)*. (Schriftenreihe des Forschungsinstituts der Friedrich-Ebert-Stiftung, number 136.) Bonn: Verlag Neue Gesellschaft. 1978. Pp. xvi, 450. DM 32.

The Labour Party in its early years functioned as a trade union pressure group on the left flank of the Liberal Party; thus it had little need for a foreign policy of its own. World War I changed all that. By the end of 1918 the Labour Party competed with Liberals as well as Conservatives in over half the constituencies of Britain. It directed its appeals not only to the industrial working class but also to idealistic middle-class voters, especially those alienated by what Labour now attacked as the undemocratic and disastrous foreign policy that had brought on one great war and that, if unchanged, threatened to bring on another.

By the summer of 1917 Labour leaders demanded a government statement of war aims beyond the simple goal of victory. In 1919 the party criticized the Treaty of Versailles for its harsh treatment of the Central Powers, and in the 1920s the party called for a conciliatory policy toward Germany, especially on reparations. Wolfgang Krieger uses a vast array of sources and his understanding of the balance of forces within the Labour Party to analyze these policies and to show how they developed. His book is not a narrow diplomatic study but a full and complex picture of the interaction of foreign policy and democratic politics. He is very good on Arthur Henderson, the one man trusted both by the trade-union leaders and by the middle-class intellectuals who began to move from the Liberal Party to Labour. Henderson provided the new recruits with a political home, the Advisory Committee on International Questions, which employed them to write reports to guide Labour leaders on foreign policy. Henderson believed that the new democratic and conciliatory foreign policy offered by Labour was both right and a vote winner. It enabled the party to appear not as the voice of narrow trade-union interests or sectarian socialism but as the party of hope and vision in foreign policy. Ramsay MacDonald, with his mastery of uplifting oratory, be-

came the symbol of this policy, but Krieger presents Henderson as its main architect.

Krieger is critical of several of the Labour foreign-policy advisers who have usually received rather kind treatment by historians. He presents H. N. Brailsford, for example, as uncritically nostalgic about the vanished Habsburg Monarchy and harshly unsympathetic toward the successor states, especially Czechoslovakia. He is very hard on E. D. Morel, who appears as a frantic apologist for everything German and an unbalanced hater of everything French. Morel reached his peak as a propagandist in "Horror on the Rhine," a pamphlet that attacked the French use of Senegalese troops in Germany as a threat to white womanhood. Morel and some of his associates presented so exaggerated a picture of German suffering and Allied mistreatment that they provided support for campaigns of the German Right, hardly a force for peace.

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MAURICE R. O'CONNELL, editor. *The Correspondence of Daniel O'Connell*. Volume 4, 1829-1832. Dublin: Stationery Office, for Irish Manuscripts Commission. 1977. Pp. 494. £12.00.

Depressing as the political events in Ireland during the last decade may be, students of Irish history can continue to derive satisfaction from the quality and quantity of work that has transformed their field since the Second World War. Long deformed and discredited by fanaticism or partisanship, Irish historiography has been revived within a single generation by the application of high standards of scientific rigor and scholarly detachment. New techniques are being employed and new aspects explored; old assumptions are being questioned and old reputations are being re-examined. In this enterprise, Maurice O'Connell has played a substantial part. With the able editorial assistance of Gerard J. Lyne, he has undertaken to assemble, collate, transcribe, and publish the complete correspondence of "the Liberator." This fourth volume of a projected eight exhibits the same attractiveness of presentation and thoroughness of annotation as its predecessors; with the recent elimination of publication problems, the final volumes soon will be available.

No individual in Irish history is more significant or more controversial than Daniel O'Connell. Loved and hated during the first half of the nineteenth century as the "uncrowned king of Ireland," O'Connell suffered a gradual decline in reputation as revolutionary nationalism rejected the British connection he had always sought to

preserve. By the centenary of his birth in 1947, the architect of Catholic Emancipation, the champion of repeal, had become little more than a stock figure of civic statuary, as irrelevant as the eighteenth-century politicians among whom he grew up. O'Connell's repudiation of violence, his intimate ties to institutional Catholicism, and—above all—his willingness to accept less than a total sovereignty for the Irish nation had made him anathema to the builders of the republic. The new Irish historiography, among its other virtues, shows a greater flexibility in defining the nature of nationalism and in confronting the question of Irish "identity," and there is abundant evidence that O'Connell can expect a more sympathetic appreciation in our day than he has received during the first three-quarters of this century. In this process of re-evaluation, the *Correspondence* will be both a stimulus and an essential tool.

The editor has drawn upon a remarkable array of public and private collections to set forth not only letters written by O'Connell (except those written expressly for publication in newspapers) but also a generous selection of letters received by him. Volume four includes some 440 items, thoroughly cross-referenced, and a useful index of persons mentioned in the letters. The period covered ranges from O'Connell's departure for London early in 1829, after his election to Parliament had precipitated the crisis of Catholic Emancipation, to the aftermath of the Great Reform Bill of 1832. A novice in parliamentary maneuvers, O'Connell made false starts and blunders, but he was also, in his mid-fifties, a veteran of rough-and-tumble Irish politics, and by the end of this volume was already forging a disciplined and formidable party among the members from John Bull's other island. The *Correspondence* as a whole will give an unparalleled view of the origins of modern mass democracy.

WILLIAM D. GRIFFIN
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GUY CABOURDIN. *Terre et hommes en Lorraine (1550-1635): Toul et Comté de Vaudémont*. In two volumes (*Annales de l'Est*, Mémoire, number 55.) Nancy: Université de Nancy II. 1977. Pp. 763.

In the past decade American readers have become increasingly conversant with a distinctive product of French intellectual life—the massive social and economic regional study that traces the relationships among a series of long-term factors deriving from the *Annales* tradition. Devotees of the genre understand that it produces a manual of very specialized data the significance of which cannot pos-

sibly be summarized in a brief review. The general reader nevertheless has the right to ask two questions of the reviewer: whether or not the latest addition to this impressive corpus of scholarship is one of those towering masterpieces worth reading for its own sake and whether it introduces any particularly interesting new method or category of insights that extends the range of phenomena explored by such studies.

The present work, like its predecessors, is a monument of grueling, workmanlike reconstruction, as indicated by its 717 pages of text interspersed with over 150 charts and graphs and by the author's awesome inscription "Nancy, 1954-1974." Admirers of the form will pore, as I did, over the many examples of social hierarchies, seigneurial revenues, price curves, and marriage patterns that have been laboriously retrieved from sleeping archives. Yet, on the whole, Guy Cabourdin's *Lorraine* strikes me as more specialized and less accessible than the most celebrated of the *Annales thèses*. It concerns the city of Toul and its geographic region, the "Toulois and the County of Vaudémont," an area that was extraordinarily fragmented administratively and that lay economically and socially halfway between the French and German patterns. This was the scene of Callot's moving engravings of peasant life: a land of "mediocre" fortunes, backward economy, and small farms; with demographic patterns more "German" than "French," grain prices more "Continental" than "Atlantic," and social crises more "biological" than "climatic." The heterogeneity of the region made it impossible to construct the sort of coherent socioeconomic system portrayed in the most famous comparable studies. The early period covered, from 1550 to 1635, was also restrictive; the author had to make do with spotty parish registers and tax rolls. The work nevertheless spans the critical era of renewed demographic pressure at the end of the "long sixteenth century" and before the catastrophic watershed of the Thirty Years' War. Cabourdin was thus able to interpret the social consequences of overpopulation and impoverishment in a manner comparable to Jean Jacquart's study of the Île de France. The results, however, are different. Cabourdin found a countryside of surprisingly vigorous seigneuries managed intelligently by absentee lords, a land teeming with venal clerics unthreatened by the Reformation, and a crisis of the 1580s and 1590s in which townspeople—even artisans—were buying up the countryside, but in which no one was consolidating either estates or seigneurial revenue farms. The population maintained itself by somewhat earlier marriage than elsewhere, and organized itself by criteria departing from the standards of "honor" set forth by Charles Loiseau (and Roland Mousnier).

The book does not contain major interpretive surprises. Its core, which I would recommend to those not wanting to read the whole opus, nevertheless offers an intriguing application of notarial archives to the quantitative study of the urban rape of the countryside (pp. 323–560). Lacking other documentation, Cabourdin recorded and classified more than 20,000 notarial contracts that he used to delineate, with great subtlety, the manipulation of credit and land ownership by the notables and the resulting fundamental shift of social structure. This familiar process has never been so clearly measured and related to specific crises. I can only hope that with his gargantuan task completed, Cabourdin will now turn to producing the shorter, more digestible synthesis for which his compatriots are also justly renowned.

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RICHARD BONNEY. *Political Change in France under Richelieu and Mazarin, 1624–1661*. New York: Oxford University Press. 1978. Pp. vi, 508. \$37.50.

Francophone historians are more and more given to writing broad treatments of French economic and social history and in their work the first half of the seventeenth century tends to get lost. At the same time Anglophone historians have been concentrating on that period of French history, and, while almost always including some social and economic material, they lean heavily toward political history. If some *conjoncture* is unfolding here, those involved have not tried to explain it. But those who have worked the same archives as Richard Bonney will realize the prodigious amount of work that went into this book.

The end result is much more information about the development of intendants as an institution. The detail is placed within a framework best expressed by the author: "Between 1624 and 1661, under the direction of the chief minister and his subordinates, the central government developed a sense of its own identity and purpose" (p. 27). Yet this framework raises problems for the reader. How aware was anyone of the process? Bonney has problems maintaining the thesis himself. "War, above all the fiscal demands of war, was to prove the decisive factor in the establishment of the intendants" (p. 36). "Commissions, instructions, and decrees," he adds, "were issued on an *ad hoc* basis, which explains something of the dynamism of the intendants under Richelieu and Mazarin" (p. 135).

The author's evidence that intendants became more and more numerous and effective in the 1630s and 1640s is undeniable, and the fact that the intendants were noticeably similar in background

and attitudes is carefully documented. But were the intendants consciously selected because of their background or was it that *officiers* were the only logical choice since they alone had the necessary training? The activities of the intendants are clearly shown, particularly their roles in tax collection and the limitation of local autonomy. And the Fronde, which it is now fashionable to see as having had a long-term effect in slowing down the development of absolutism, is restored to its former place as an interruption in a long process. But again, how conscious was the process?

The author, through diligent work, has given definitive answers to the questions of what the intendants did and how the institution developed during the middle years of the seventeenth century. Further, he presents some convincing arguments that the Fronde was an interruption in French history. He does not, however, show the extent to which absolutism was more than a by-product of French foreign policy.

Some repetition and roughness of organization mar the book. The actions of the Parlement of Paris in the spring of 1615 are once again misunderstood. The postscript on the personal reign of Louis XIV is based more on conjecture than archival research. And was Bossuet really "the greatest Catholic mind of the Age" (p. 431)? Yet this book will be a mine of facts for all those interested in the first half of the seventeenth century in France. Even French historians of France will have to read it.

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JEAN MEUVRET. *Le problème des subsistances à l'époque Louis XIV*. Volume 1, *La production des céréales dans la France du XVII^e et du XVIII^e siècle*. In two parts. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 50.) Paris: Mouton. 1977. Pp. 223; 222.

Pierre Goubert is undoubtedly correct in asserting in the introduction that if this book had appeared in 1952 it would have had an extraordinary impact. The present volumes and those to follow are the posthumous publication of part one of Jean Meuvret's projected three-part thesis on the problem of subsistence in Old Regime France. Meuvret never completed his thesis. In 1952 he finished the general introduction and the first section on the technical aspects of grain production and their social setting. When he abandoned the project in 1955, he left a virtually complete draft of the second section on the grain trade, but only an outline of the third part on the monarchy's role in handling food supply. Meuvret continued his research and published

some thirty articles and books, but for all that he is probably the least well-known of the major scholars in the *VI^e Section-Annales* group.

There can be little doubt that Lucien Febvre, Fernand Braudel, C. E. Labrousse, and many younger scholars overshadowed Meuvret. He shared none of their enthusiasm for self-conscious pronouncements on interdisciplinary history. He knew too much economics to be taken in by Pierre Chaunu's serial history techniques, and he never indulged in the cosmological musings so typical of the *Annales* historians. In fact he was out of phase with his major colleagues. Meuvret was a meticulous researcher in economic history, never hasty to rush into print. He wrote without ideological bias in a clear and careful style that was decidedly old-fashioned. Virtually none of the harsh criticisms now being leveled against the *Annales* historians, with good reason, apply to him.

The key to Meuvret's work is found in the general introduction to the present volumes. He had a comprehensive view of Old Regime agriculture, rural society, and economic institutions. For him the central problem was the subsistence crisis so apparent in Louis XIV's France and its disappearance by the middle of the eighteenth century. To understand the mechanisms at work, Meuvret explored the entire socioeconomic, institutional, technological, and political environment. The techniques of grain farming, soil chemistry and agronomy, agrarian economics, horticulture, property distribution, financial institutions, rural social structure, the market mechanisms of the urban grain trade, price history, and government policy, to mention only the major topics, were all seen as part of an interlocking system. The retreat of the subsistence and mortality crises pointed to a major shift in the entire system. Meuvret insisted that the key to understanding this change lay not in this or that element in isolation, but in the interrelationships and covariance of all the factors.

This was a remarkable insight for 1952 and a major advance over Marc Bloch's understanding of the rural world. Unfortunately, it was precisely this grasp of agriculture and the rural world as an integrated system that Meuvret sacrificed by publishing his findings in dribs and drabs over thirty years. Meuvret never solved completely the problem of the subsistence crises in all their ramifications; the project was simply too big for one scholar. But his vision of the interlocking mechanisms of the rural economy, a "system's approach" before the term became popular, is as much needed today as it was in 1952. French rural history, an amorphous sprawl that is more social than economic, still has much to learn from the late Jean Meuvret.

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B. ROBERT KREISER. *Miracles, Convulsions, and Ecclesiastical Politics in Early Eighteenth-Century Paris*. Princeton: Princeton University Press. 1978. Pp. xvii, 485. \$27.50.

In the halcyon days of the 1960s, when expansion was the order of the day and publishers combed the halls of academe in search of budding talent, the young historian found no difficulty in breaking into print. How many theses eventually wound up, as a result, being remaindered, no one presently knows. No doubt some computer-oriented confrère will give us the answer one day.

Much as I hate to say so, some advantage may derive from the current economic crisis. It is no longer so easy to publish a thesis. Despite the obvious difficulties this creates for enterprising and deserving scholars in terms of personal satisfaction and career advantages, it may turn out to be just as well.

These remarks are prompted by a reading of B. Robert Kreiser's book, which, I take it, is an expanded version of his doctoral dissertation. (Although nowhere explicitly stated, the internal evidence, notably the acknowledgements, indicate that this is so.) Let me say at once that I have no animus against either the author or the book. It is competently researched and clearly, if not excitingly, written. Kreiser may have the satisfaction of knowing that no one need retrace his footsteps in the extensive archival sands he has traversed. I am simply asking: Why bother in the first place? If the answer invokes the fun of the chase, the pleasure of detection, then I can only reply *de gustibus* and let it go at that. But I suspect that Kreiser, like most of us who aspire to touch the hem of Clio's garment, had another reason for undertaking this research, namely to add to our historical understanding. If, as I think, he has overshot the mark, it is not because he lacks ability, but rather because he has got caught up in a subject whose importance is simply not that great.

Kreiser situates his subject at the intersection of ecclesiastical politics and popular piety in Paris between 1727 and 1735. By that time the Jansenist controversy had been going on for well over half a century, Port Royal had been raided and *Unigenitus* had become the law of the land. Not everyone was happy with this turn of events. The lines of combat within what it is now fashionable to call the elite had been drawn—the king and his ministers supported by the Jesuits and the bulk of the church hierarchy on the one side, the die-hard Gallicans, Jansenists, and Richerists on the other, *appellants* versus *constitutionnaires*. The issue had long since ceased to be (if ever it had been) a purely intellectual debate about the efficacy of grace and frequent communion. What mattered now were questions of social control and political

power. Whose church was this anyway? And who was to decide what its members (everyone, by definition) should believe? The conflict is thus an episode in the disaggregation of a ruling class played out within the specific institutional context of the era: Church versus state, national versus international authority, hierarchy versus lower clergy. Curiously, Jansenism, despite its inherent potential, never became the vehicle for bourgeois self-assertion in France that Calvinism did in England or the Netherlands. Deism and various forms of rational religion played that role instead. This being the case, the active intervention of the *menu peuple* into the controversy consequent upon the miracles of Saint Médard could only be of limited importance. No doubt the mass audience (how many, in fact?) for the miracles pointed to a certain dissatisfaction with conventional religious outlets among the lower classes. That was not something very new. They had no desire, it would seem, to abandon the True Church. Not even the *convulsionnaires*, much more limited in numbers, did that. The Gallicans and the Parlement de Paris might seize the occasion to express their dissatisfaction with Cardinal Fleury and Archbishop Vintimille, but they knew, as the French say, just how far not to go too far. One has only to look at the Parlement's lukewarm defense of Carré de Montgeron for proof of the assertion. To involve the *menu peuple* in a challenge to the authority of the ruling class would have been going too far. As I have suggested in my *Names of Kings* (1972), the *menu peuple* had not yet formulated, even in religious terms, an egalitarian ideology capable of posing a real threat to the monarchy and the hierarchy that supported it, however much the premises of such an ideology may have existed in the fraternal practices of *frères* and *soeurs* in the *convulsionnaire* conventicles. So long as there was no one outside their ranks to mobilize them in drawing out what Kreiser calls the "full implications of their spiritual and ecclesiastical position," their religious practice would remain marginal to the process of social change.

Kreiser sees his subject as an episode in a kind of long-term crisis of old regime society, but what the nature of that crisis was, beyond a contest of elite groups for a share in "authority" (not, apparently, "power," an important difference since the two words are not synonymous), he never states. But until he develops his vision of what was at stake and who the players in the game of high politics were, he cannot justify the importance he attributes to this tempest in a Parisian cemetery. His conclusion is a lame one, when he writes that the "stormy religious crisis of the 1730s . . . seems not only to have revealed, but also to have exacerbated some of the fundamental . . . tensions of the ancien regime and portended serious trouble both for the

integrity of the Gallican Church and for the very future of Bourbon absolutism" (p. 401). In fact, despite Kreiser's strictures (p. 275), Fleury and the crown met the challenge of the 1730s pretty well, cleverly playing their adversaries off against one another and engaging in only a very limited sort of repression. There was no overkill, for the circumstances did not warrant it. I cannot see that the episode allows us to characterize old regime France as "a society where most authority, secular as well as ecclesiastical, was more apparent than real" (p. 401). Nor am I sure that established religion had a "dysfunctional character" in eighteenth-century France, at least for the *menu peuple*. "Dysfunctional" makes sense only in terms of a goal to be accomplished. What was that goal?

At the end of an exhaustive chronicle that leaves no ceremony unperformed and no confrontation unreported, our understanding neither of popular religion (of which many unexceptionable and not very enlightening things are said) nor of ecclesiastical politics is notably enhanced. We get a description of their interaction, but not much more. I am not sure that the *convulsionnaires* can be lifted out of the domain of the *fait divers*, but, in any case, this book has not done so.

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GEORGE D. PAINTER. *Chateaubriand: A Biography*. Volume 1, (1768-93) *The Longed-for Tempests*. New York: Alfred A. Knopf, 1978. Pp. x, 327. \$15.00.

This is the first volume of a projected three-volume biography of a member of France's first generation of Romantics, François-René de Chateaubriand (1768-1848). Given the fact that its subject lived through the most crucial decades of modern French history and participated in all three of the period's revolutions (1789, 1830, and 1848), this excellent biography should be of interest to all students of French history and literature since 1750. Should the next two volumes of this endeavor fulfill the promise of *The Longed-for Tempests*, George D. Painter will have given us one of the most learned and complete biographies of a Frenchman of letters in English since his own earlier work, *Marcel Proust: A Biography*.

This volume begins, literally, with the birth of the future politician and author of the almost unbelievably influential *Atala* (1801), *René* (1802), and the polemical *Génie du Christianisme* (1802). In the first chapters of the book Painter scrupulously recreates Chateaubriand's brooding childhood in the melancholy province of Brittany, giving an ample history of his family, a perfect example of the *petite noblesse*. It was, however, the social and political upheavals of the 1780s and 1790s that formed Chateaubriand and, in a way, freed him

from the obligations of his family's mediocre name and responsibilities. In his portrayal of the gradual disintegration of the Breton nobility's reaction to Louis XVI's reign into a popular and violent revolution, Painter shows that Chateaubriand was close enough to these events to see death and to know fear. For these and other reasons, he decided, at the height of the troubles, to take a long-contemplated trip to America.

This section (part three, "The Noble Savage") is the most controversial of the book. Everyone who knows anything about Chateaubriand knows the questions surrounding his eight-month sojourn in America in 1791. There is no doubt that he went to America, but there is considerable debate about his peregrinations there. So many inconsistencies have been found in his writings by so many dedicated Chateaubriandists that, until Painter's reconstruction, it was generally accepted that the young traveler had wandered only on the eastern coast of the new republic, never venturing farther west than Niagara Falls. Yet through a meticulous study of Chateaubriand's letters and other writings as well as of contemporary travel accounts and records, Painter convincingly reconstructs his subject's purported itinerary from the Niagara River along Lake Erie, south to Pittsburgh and down the Ohio River to Cincinnati, and then beyond to where the confluence of the Ohio, Tennessee, and Mississippi Rivers provided a stunning vista of this new and rich nation. Returning by way of Nashville and Knoxville up through the Blue Ridge Mountains and the Shenandoah Valley, Chateaubriand reached Philadelphia in November 1791. The entire trip, calculates Painter, covered 3360 miles in 116 days, a feat quite possible since at least a third of the voyage was on fast-moving rivers. Concludes Painter: "So we have established for the first time, disentangling the obscurities and errors of his narrative, the route which [Chateaubriand] claims to have taken and endeavoured to describe" (p. 218). This is an important claim, for, if Painter is correct, a new reading of Chateaubriand's works is called for, one which would suffer no further interference from attacks on the author's credibility and veracity.

The volume ends with a description of Chateaubriand's service in the *émigré* armies and his departure from a terror-ridden France to exile in England. The next two volumes of *Chateaubriand* may not be as controversial as this one, but if they are as well written and researched, they will certainly be equally compelling.

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PHILIP A. BERTOCCI. *Jules Simon: Republican Anticlericalism and Cultural Politics in France, 1848-1886*.

Columbia: University of Missouri Press. 1978. Pp. vi, 247. \$17.00.

These days Jules Simon is hardly a name to conjure with. Most European historians would be hard-pressed to associate him, even vaguely, with the early, troubled years of the Third Republic. In the eyes of his contemporaries Simon appeared, for at least thirty years, as a major figure: professorial defender of the Napoleonic university against church encroachment; philosopher preaching a tolerant theism to an educated public; republican conscience challenging the Second Empire's authoritarianism; moderate republican leader vainly seeking to forge a conservative alliance; and opponent of "excessive" anticlericalism who took on, in succession, Léon Gambetta and Jules Ferry.

In what seems to be the first full-length biography of Jules Simon published in this century, Philip A. Bertocci has proposed to dispel our ignorance. This very competent monograph deals with the development of Simon's ideas, their reception by the French intellectual and political elite, and the link between Simon's intellectual stance and his political activity. The focus is on Simon's carefully thought-out position on the proper republican attitude toward God, liberty, and the church. The emphasis is less on the well-known conflict between clericals and anticlericals than on the sharp philosophical and political differences that distinguished a moderate "spiritualist" like Simon from his Voltairean, positivist, and pantheist colleagues. To the extent that historians have tended to think of French anticlericalism as a bloc, Bertocci's book will force them to reconsider, and that alone is a substantial service to the profession. Simon's career demonstrates that anticlericalism was compatible with a sympathetic understanding of the Catholic faith.

Yet a biography of Simon has its pitfalls. In the first place, Jules Simon, heir to Victor Cousin's eclecticism, was a facile but not an arresting thinker. However widely read his disquisitions on religion may have been in the 1850s, they have not aged well, nor do they gain in the retelling. Second, Simon had the bad luck of articulating his own theistic position at the very moment when a rising generation embraced positivism. Simon could find many readers, but few intellectual peers took his ideas seriously. Third, Simon's career as a politician—whether in opposition or in power—was singularly unproductive. Bertocci is inclined to ascribe this to circumstances rather than to incompetence, but as another eminent politician has recently noted, "life is unfair." The fact remains that Simon accomplished little. The details of his political career are consequently liable to produce a certain lassitude in even a well-intentioned reader. Finally, and here the author

rather than his protagonist may be faulted, Bertocci's approach—abstract ideas as cause, political action as effect—has an extreme idealist bias. I do not wish to underestimate the power of abstract ideas, but I get nervous when their omnipotence is accepted as the only serious hypothesis. Now it may be, as the author implies, that Simon was at pains to distinguish between a “friendly” Catholicism and an unacceptable clericalism as the direct outgrowth of his philosophical position on liberty and theism. Yet is it not equally plausible that as a staunch social conservative he needed the cooperation of propertied Catholics to keep Gambetta's *couches nouvelles* from overpowering the defenders of the citadel and that he would have taken this political stand regardless of abstract ideas?

Despite my misgivings about unqualified historical idealism and my skepticism about Simon's intellectual and political stature, I do think Bertocci has opened an interesting window on what the nineteenth century saw as the crucial drama. As a study of one significant aspect of the struggle between secularism and religion, *Jules Simon* is well worth reading.

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JOHN M. MERRIMAN. *The Agony of the Republic: The Repression of the Left in Revolutionary France, 1848-1851*. New Haven: Yale University Press. 1978. Pp. xxxvi, 298. \$29.00.

In December 1868 Karl Marx wrote to Engels of his delight with Eugène Ténot's just published *La Provence en décembre 1851*. Marx thought the book important because “it is a necessary result of every victorious reaction that the causation of the revolution and especially of the counterrevolution should pass into utter oblivion . . .” John M. Merriman's history of the counterrevolution of the Second Republic is assurance that those who opposed this reaction will not be lost to historical memory. He confirms Marx's further observation, of December 19, to Engels, “The (republican) movement among the country people was much bigger than we knew.”

Merriman argues that the revolution of 1848 heightened the consciousness of working people and transformed their mobilization, with that of the rural peasants, into the cause of the “*démocrates*” (*Montagnards*). This mobilization enlisted the press, political propaganda, clubs, banquets, carnivals, *charivari*, funerals, songs, and cafés. It called forth leaders from the ordinary people who replaced prerevolutionary radicals no longer willing

or able to keep up the struggle. The telling of this story complements the works of Philippe Vigier, Maurice Agulhon, and Alain Corbin. Merriman acknowledges his debt to Ted W. Margadant's forthcoming, and much awaited, *The French Insurrection of 1851*. In this elite company no one can be completely original, but Merriman's history is nonetheless very important.

His exemplary use of the national and departmental archives renders concrete the means by which the government destroyed the social democratic leadership, press, clubs, voluntary associations, and political festivities. Merriman's account of the political transformation of the mutual aid societies is particularly good. The lists he brings from the archives, of the purge of mayors and deputy mayors from January 1, 1849 to February 28, 1851, and the record of the municipal councils dissolved from April 18, 1849 through February 28, 1851 provide critical evidence of the social democrats' frustration and destruction.

To test in depth how this multiple repression succeeded in different areas of France, Merriman examines the urban society of Limoges and the department of the Nord. This analysis is matched by attention to revolutionary hope and its suppression in Creuse, Ariège, Finistère, and Yonne. In Limoges the socialists' astonishing success in the elections of May 1849 encouraged, not unrealistically, the dream that the expected presidential election of 1852 would bring France a democratic and social republic. But by the beginning of 1851 no concerted public action was possible and the opposition of Limoges to the coup of December 2 was feeble.

The mutation of traditional kinds of peasant protest made possible more modern forms of resistance in rural France. But the people of Creuse and Ariège could not prevail against the unremitting pressure of the public authorities to stop all threats to the unrevolutionary settlement of 1848. And Finistère caused the authorities little anxiety. Yonne just managed to keep going its clandestine organizations and joined its neighbors in Nièvre during the resistance to the *coup d'état* of 1851.

Without a full commentary on the rich details of his history, Merriman concludes that the “radicalization and repression” of the Second Republic offered France the secrets of a police state; advanced working-class consciousness, political theory, and practice; and enhanced bourgeois appreciation of permanent counterrevolution. To accent this moment and its accomplishments is a considerable feat. Merriman's history merits the praise Marx paid to Ténot: “one must possess a copy of a thing of this kind.”

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LOUIS-MARIE GOREUX. *Agricultural Productivity and Economic Development in France, 1852-1950*. With revised French version. (Dissertations in European Economic History.) New York: Arno Press. 1977. Pp. vii, 122; 328-376. \$18.00.

WILLIAM HENRY NEWELL. *Population Change and Agricultural Development in Nineteenth Century France*. (Dissertations in European Economic History.) New York: Arno Press. 1977. Pp. xvii, 199. \$20.00.

The evolution of the French economy continues to intrigue economic historians. Almost thirty years after the well-known exchanges among such people as Landes, Gerschenkron, Cameron, and Sawyer, the output of scholarly work continues apace. English, French, and American scholars are active, and gatherers of new empirical materials have happily joined the many interpreters and critics.

For a time, interest in the French case stemmed mainly from the apparent slowness with which industrialization, or modern economic development, proceeded as compared with the experience of other major nations. Some thought, for instance, that the political and social strains of France—the stalemated society—owed much to this hesitating and incomplete transformation of the economy. Attention focused both on the industrial and the agricultural sectors, on small firms and peasant farms, for the most part family-run.

This interpretation has, of course, been much debated, but it is fairly clear that France's originality stems less from developments within the principal economic sectors than from the slow structural change between them, the shift from a largely rural to a primarily urban economy. And, as most students have come to accept, the key to the story is demographic. In the view of some, France merely anticipated the transition to modern demographic behavior. Others interpret the French experience as bypassing the demographic transition altogether, with declining birth rates a direct and lasting answer to population pressure rather than a delayed response to falling death rates. Yet the single most remarkable aspect of modern French history in this regard is the reluctance of its people to participate in the huge *Völkerwanderungen* that so changed the spatial distribution of European peoples. Rates of internal as well as overseas migration were much lower than in other countries. Thus one can almost interpret low marital fertility as a forced response in a densely populated rural society that had rejected the alternatives of rapid economic intensification and migration. In turn, of course, slowly growing population and towns exerted a drag on the economy. The high road to rapid investment and in-

novation—fast-growing domestic demand—was closed to French entrepreneurs.

With the foregoing as background, we can examine the volumes under review here. At a sixteen-year interval, the two authors studied change in rural France during overlapping portions of the modern period. Both strove to combine agricultural and demographic dimensions, and both have previously published some of their findings in article form. Thus the reviewer's task is not only to compare and comment on these works, but also to ask what the reprinted dissertations add to the store of available useful knowledge.

William Henry Newell's principal contribution is to document the diffusion of improvement in rates of agricultural output from north to south within France, focusing on the period roughly from Waterloo to Sedan. He attributes the apparent gains (the data have been questioned, as he notes) to the spread of mixed farming. More dramatic agricultural innovations, such as mechanization, had no impact then and little until very much later; it appears that progress tended to slow rather than speed up late in the century. As for population, Newell engages in some suggestive analyses of relations among components of demographic change; these chapters have not, to my knowledge, been published. He is unsuccessful, however, in combining economic and demographic variables into a coherent picture of rural France.

This integration forms the key to the work Louis-Marie Goreux completed in 1955. His model, and it is a formal one, begins with spatial differences in agricultural productivity that translate into wage differentials. These in turn set up a potential for labor migration, the rate depending not only on the size of the wage gap but also on the distance of the area in question from centers of economic development. All would be straightforward, were it not that technological change in agriculture continues to favor high-productivity areas, thus retarding the effect of migration in closing the wage gap. Indeed, given the French reluctance to migrate, economic differences tended to widen for a time before they began to close. Despite their limitations, the data provide good support for this theory.

This is an impressive piece of work, though little noticed by economic historians and originally done under the sponsorship of specialists in agricultural development. Most satisfying is Goreux's systemic approach, in which rural change responds to urban-industrial development as reflected in wage gaps. The thrust of the model and its findings tend to contradict Newell's conclusion, at least for the period of overlap. The point is of some wider significance, since the last quarter of

the nineteenth century was marked by a return to agricultural protection, and it is important to know whether this policy was conducive to agricultural development and, if so, whether it favored lagging or leading regions.

As already mentioned, this work too has been published, in French and in a periodical not readily accessible to many American academics. But the editors of the dissertation reprints have had the fine idea of appending the French article to the original English text. It reads better, with useful maps and charts (although the shading on some of the maps is confusing) and with the algebra relegated to appendixes.

This brings up a final point about form. We must welcome reprint efforts that make dissertations more widely available. But the reader, particularly the nonspecialist who benefits most, also pays a price. Readability is not high on the list of requirements for most doctoral dissertations. With changes in intellectual fashions and the monetary realities of publishing, there will continue to be a place for raw dissertations in scholarly libraries. But the temptation to have one's work instantly published should not discourage young scholars from making the effort to refine and rework the material and to find a more traditional outlet for it.

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PHILIP CHARLES FARWELL BANKWITZ. *Alsatian Autonomist Leaders, 1919-1947*. Lawrence: Regents Press of Kansas, 1978. Pp. xii, 203. \$12.50.

Philip Bankwitz's studies of civil-military relations during the last years of the Third Republic established his scholarly reputation as one of the truly front-rank historians of modern France in this country. It is thus sad to report that his latest book—a study of five Alsatian autonomist leaders during the period 1919-47—is such a disappointing effort. Thin, repetitious, and lacking a significant thesis or argument of any kind, this slight volume is nonetheless burdened by and finally sinks under the weight of a scholarly apparatus that occupies fully one-quarter of its 171 pages. A dense bibliography of twenty pages follows.

It is the narrow focus of this book that accounts for its shortcomings. Bankwitz states in his introduction that he intends only to recount the post-1919 careers of five Alsatian autonomists—Joseph Rossé, Jean-Pierre Mourer, Marcel Stürmer, Hermann Bickler, and Friedrich Speiser—as well as those of Robert Wagner and Robert Ernst, two German officials deeply involved in Alsatian autonomist affairs before and during the Second

World War. Thus, the author frankly declares that he has not attempted a history of the autonomist movement nor an analysis of the region or economy of Alsace, and he goes on to say that his book is not even a history of Alsatian autonomism during the years 1919-47 as seen through the local and international press. But it is precisely such studies and analytical orientations that are needed if the political careers of Alsations like those Bankwitz examines are to be made intelligible. As it is, Rossé, Mourer, and the other Alsatian militants surveyed in this book emerge as little more than pasteboard figures whose lives and careers seem more passive than active forces in the gathering drama of Alsatian history during the interwar decades and the years of German occupation from 1940 to 1945. Hence, all that Bankwitz can claim by way of conclusion—inappropriately described as the thesis of his book—is that “the interwar Alsatian autonomist leaders were failures and that by 1947 their movement was reduced to ashes” (p. 125). One may legitimately wonder if a full-length monograph is really necessary to arrive at so unmomentous a finding since it is generally known that Alsatian autonomists of the variety studied by Bankwitz, gambling as they did on the possibility of ultimate German victory in the Second World War, were doomed to failure when that outcome did not materialize.

It is unfortunate that a man of such imposing scholarly credentials as those possessed by Bankwitz should have produced so inadequate and disappointing a book as this one. And his failure to cast his investigative and conceptual net wider or to attempt a more systematic history of the Alsatian autonomist movement while relating it to an analysis of political, social, and economic life in Alsace cannot be justified, as Bankwitz seeks to do, on the ground that relevant Ministry of the Interior archives are closed to researchers. Frustrating as this situation certainly is, it does not pose an insuperable obstacle (as I discovered in the course of my own work on the very similar Breton autonomist movement during the same period) to the completion of that far more ambitious and wide-ranging study that post-1919 Alsatian autonomism surely deserves.

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H. R. KEDWARD. *Resistance in Vichy France: A Study of Ideas and Motivation in the Southern Zone, 1940-1942*. New York: Oxford University Press, 1978. Pp. ix, 311. \$21.00.

Resistance in World War II is an irresistible but refractory subject. Henri Michel's *The Shadow War*

(1972), the most satisfactory survey, calls resistance documentation "a real jigsaw puzzle" of false papers, double identities, exaggerated claims, and a myriad of obscure individual actions (p. 255).

H. R. Kedward has avoided some of these difficulties in *Resistance in Vichy France* by narrowing his inquiry. He deals with movements that sought to influence the public, usually by leaflets or illegal newssheets. This leaves aside the intelligence, escape, and sabotage networks, and, since he stops in 1942 with the full occupation of the Vichy zone, he does not assess later military impact. This is essentially a study of beginnings: why and how did French people in the Vichy zone first break with the prevailing apathy and orthodoxies?

Kedward strips away postwar assumptions that the correct path was apparent at once to true patriots in June 1940 and reconstructs the uncertain gropings of the first days. He finds the first steps a matter of wide individual variety, "beyond categorization" (p. 79), without any one determinant such as social class or outsider status. This kind of stress upon personality and circumstance often lapses into a vague accidentalism, but Kedward has superlative pages on how contacts spread more easily among some professions and milieus than among others. There is also an intelligently argued discussion of the Communist position before June 1941 as genuine resistance to Vichy without participation in the "imperialist" war effort.

Kedward attributes more to the push of Vichy's vindictive partisanship than to the pull of such external encouragements as the entry of Russia and the United States into the war. Anger played a greater role than hope in this account, as Vichy antagonized schoolteachers, Jews, freemasons, and finally some Catholics. This analysis applies strictly to the Vichy zone, of course, where the absence of the Germans and the legitimizing presence of Pétain gave a particular pace and tone to resistance; it would not apply to the period after 1942.

Kedward has made skillful use of interviews as well as of the underground press. His main research gap is German materials. What he has drawn from partisan French accounts of army weapons caches, for example, would be far more convincing if we were told what the Germans found after November 1942. It would also help to know when and in what form the Germans first noticed resistance in the Vichy zone. Otherwise we risk taking the resisters' postwar self-estimates at face value.

Kedward's main achievement has been to recreate the uncertainty, variety, and spontaneity of the early resistance in the Vichy zone, and to lay out the stages by which the Communists' opposition to Vichy (but not to the Germans) and the moder-

ates' opposition to the Germans (but not to Vichy) merged into a single movement by 1942. This is a cool, detached work free of the false certainties and the derring-do of much of the genre.

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ANTONIO COLLANTES DE TERÁN SÁNCHEZ. *Sevilla en la Baja Edad Media: La Ciudad y sus hombres*. Seville: Sección de Publicaciones del Excmo. Ayuntamiento. 1977. Pp. xv, 447. 700 ptas.

This demographic history of the city of Seville in the fifteenth century is a nearly flawless example of its genre. Antonio Collantes de Terán traces changes in the size, legal status, social composition, and wealth of the city's population by analyzing the *padrones* (tax censuses) from 1384 through 1533. He finds that the number of *vecinos* increased from 2,613 in the defective *padrón* of 1384 to 9,660 in 1533, with the most spectacular growth occurring between 1450 and 1480. For those sectors of Sevillian society not subject to the municipal taxing authority—and therefore usually not included in the *padrones*—Collantes de Terán turns to supplementary sources such as municipal ordinances, royal decrees, military rolls, church records, and notarial registers.

The author estimates that there were between 450 and 500 *vecinos* in the Jewish ghetto at the end of the fourteenth century and that about one-sixth of them died or emigrated in the riots of 1391. By 1437, the combined Muslim and Jewish population of the city had dropped to 121 households and only 25 Jewish households were left by 1482, when the Jews were expelled from Seville. Much of the early decline must be attributed to conversion and the later to emigration, for the number of *converso vecinos* swelled to about 250 between 1391 and 1451 and then declined to less than 80 in the 1484 *padrón*, after the establishment of the Inquisition.

After surveying other exempt populations—most significantly the clergy and wholesale merchants from northern Spain and Italy—Collantes de Terán analyzes the upper ranks of Sevillian society, the *caballeros* and the *francos*. In 1384, 30 percent of all *vecinos* were listed as *caballeros*, ranging all the way from titled noblemen to tavern keepers. In the *padrones* for the 1480s a new category—*hidalgos*—emerges and, by 1542, 138 *caballeros* and *hidalgos* were counted in Seville. The *francos* were persons who belonged to the social status of taxpayers, but who had royal or municipal tax exemptions because, it was argued, as public employees they were paid lower salaries than the prevailing rates in the city. In 1384, there were about 600 *francos* (about 23 percent of the popu-

lation) who were principally employees of the royal mint and shipyards, municipal employees, and seamen subject to naval duty in war. A special tax exemption to textile workers granted by the crown to encourage the industry in Seville—which had no textile industry in the fourteenth century—appears to have been successful: in the 1384 *padrón* 166 *vecinos* were declared *francos* because they worked in textiles, although the *padrones* of the mid-fifteenth century do not mention such an exemption and the number of households in the textile industry had increased to 651 by 1483 and to 993 by 1533.

A third group of *francos* was made up of *familiares*—dependents of secular clergy—but their numbers declined from 242 in 1435 to 49 in 1516. The number of slaveowners increased significantly between 1483 and 1512—from 4.8 percent to 8.9 percent of the *vecinos* in their respective parishes. Slaveowners came from 50 different trades and professions, as well as from the nobility, and ranged in wealth from the richest man in the city to householders so poor that they had nothing of value to declare except a slave. The distribution of wealth was as unequal as we would expect, and it changed little during the period.

This work is impressive in its wealth of detail, analysis of the strengths and weaknesses of each source, mastery of the statistical and methodological procedures, as well as command of the primary and secondary literature. Equally admirable is the author's restraint: he does not attempt to pull from the sources what is not there—he does not, for example, speculate on size of household, male-female ratios, or life-cycle patterns—and does not apply quantitative methods to samples too small to be valid for statistical analysis. Consequently, the few conclusions modestly advanced by Collantes de Terán can be taken with the greatest confidence. The most salient is that, while the increase in slaveholding between 1483 and 1512 can reasonably be attributed to Seville's role in the new American trade, its famous population boom in the sixteenth century must be seen as the continuation of a pre-discovery pattern rather than something new. The book is rich in conclusions of lesser scope but equal significance and interest—about changing sources of wealth, geographical variations in growth within the city itself, the decreasing number of tax exemptions, and the shrinking number of *caballero* families controlling the municipal government. From the opening description of the physical city, redolent with stagnant water supplies and inadequate sewage facilities, to the final analysis of individual fortunes, the work is a masterpiece of urban history.

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WALTHER L. BERNECKER. *Anarchismus und Bürgerkrieg: Zur Geschichte der Sozialen Revolution in Spanien, 1936–1939*. (Historische Perspektiven, number 10.) Hamburg: Hoffmann and Campe. 1978. Pp. 372. DM 68.

This is the most thorough and systematic treatment to date of the socioeconomic revolution attempted by Spanish anarchosyndicalists of the CNT during the civil war of 1936–39. Walther L. Bernecker has taken advantage of new archival materials, incomplete though they are, that have been opened for investigation in the 1970s and has made use of nearly all available sources, primary and secondary. The book's organization is logical and conceptually sophisticated, proceeding from a discussion of leftist politics to the two main divergent interpretations of the revolution (anarchist and Communist) and concentrating on three lengthy chapters that deal with "Collectivization in the Agrarian Economy," "Collectivization in Industry and Service Enterprises," and "State and Revolution."

The two sections on collectivization constitute the core of the book and provide a concise and systematic exposition and analysis of the processes, structures, and main problems of anarchosyndicalist collectivization. Bernecker is particularly effective in explaining the formal procedures and legitimization processes of the revolution, since these can be more directly documented than the actual social and economic performance of the anarchist collectives, from which comparatively little direct evidence has survived.

Though sympathetic to the alternate "libertarian" revolution of the anarchists, Bernecker is frequently critical of the course of the revolution and particularly of certain policies of the CNT leadership. He emphasizes repeatedly the lack of any coherent, well-conceived plan on the part of the FAI-CNT organization for coping with the political and economic problems of the revolutionary process. Originally conceived as a syndical and communal revolution, the movement in fact developed almost spontaneously through the formation of individual collectives that were never effectively coordinated into one practical, overarching revolutionary system or strategy capable of dealing with Republican moderates or the Communist power drive. Global statistics on the collectivization process cannot easily be determined (if at all), and Bernecker leans toward the high side, estimating that in one way or another as many as three million people may have been involved. In general he credits the collectivization of the municipal service industries with relative economic success and the agrarian collectives with mixed or partial economic success. He recognizes that the industrial collectives constituted the most negative case,

though emphasizing (no doubt correctly) that no valid economic judgment on the anarchist collectives can be rendered, given the limited time-span and highly disturbed economic relations of the Spanish revolution. He believes that their greatest achievement was not economic (and certainly not political) but social and cultural, for he credits them with having largely "overcome alienation" and established positive social relationships. He avoids discussing the darkest side of the anarchosindicalist revolution, large-scale participation in the Red Terror.

The villain of any consideration of the anarchosindicalist revolution in Spain is almost always the Communists, and this study is no exception. Yet one must be careful to give the devil his due, and there is some doubt that Bernecker has managed to do that. His analysis perhaps renders less than justice to the Communist strategy of a controlled and "responsible revolution," which was probably not nearly so counterrevolutionary (though indeed counter to the anarchist revolution) as Bernecker describes it. This is, however, a common failing of many accounts of the Spanish war, including the recent general interpretive history by Raymond Carr.

Certain issues of perspective and interpretation aside, this should become a standard account of the structures, processes, tribulations—and possible achievements—of the anarchosindicalist revolution in Spain.

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HUGO SOLY. *Urbanisme en kapitalisme te Antwerpen in de 16de eeuw: De stedenbouwkundige en industriële ondernemingen van Gilbert van Schoonbeke* [Urbanism and Capitalism in Antwerp in the Sixteenth Century: Town-Development and the Industrial Undertakings of Gilbert van Schoonbeke]. (Historische Uitgaven Pro Civitate, number 47.) Brussels: Gemeentekrediet van België. 1977. Pp. 496. 700F.

In mid-sixteenth-century Antwerp, at the peak of the city's economic importance, a certain Gilbert van Schoonbeke was active, first as a real-estate speculator and developer, and later as an industrial entrepreneur and government contractor. In his brief career from 1544 to 1556 (he died at age thirty-nine) his real-estate ventures changed the face of parts of Antwerp. He developed the largest (and last) addition to the pre-nineteenth-century city; he organized Antwerp's largest public works project, the construction of the city walls and gates; he gained control of the city's brewing in-

dustry and completely reorganized it; he leased collection of the city's major excise taxes; and he built up a vertically integrated building materials supply operation. At the peak of his career he monopolized construction, real-estate development, and the brewing industry in the city and was an influential adviser to the governor of the Low Countries.

Hugo Soly's *Urbanisme en kapitalisme te Antwerpen in de 16de eeuw* is a detailed study of Van Schoonbeke's career, but it is also more. Through his subject's career Soly seeks to analyze the nature of sixteenth-century urbanism and early capitalism. The book is divided into three parts: the first describes Antwerp's sixteenth-century real-estate market and its techniques; the second and largest part details the activities of Van Schoonbeke from his humble beginnings through his amazingly varied and successful career to its dispirited end (in 1554, riots directed at him undermined his position, and upon his death city officials, even more rapacious than he had been, despoiled his estate). The last part assesses the historical meaning of Van Schoonbeke's career. He was no ordinary developer and monopolist. Soly argues that his real-estate development realized an urban vision that was absent in the other growing cities of the sixteenth century (Rome alone excepted) and that, since his industrial enterprises showed an organizational ability and insight that did not survive him, he was ahead of his time. Because he was so exceptional, Soly concludes, Van Schoonbeke was a historically marginal figure.

Soly knows the archives of Antwerp like few other historians have ever known them. One is repeatedly amazed at how much it is possible to know about Van Schoonbeke's activities and how Antwerp's residents responded to them. Still, at points Soly fails to convince, or seems to go well beyond his evidence. Despite all of the details, Van Schoonbeke's personality does not come alive to the reader. Instead, we find the author repeating far too often his assertions about his subject's vision and intentions. Van Schoonbeke enjoyed close relations with the city government, which he encouraged with bribes and shady deals. In time he was done in by his former co-conspirators. Soly describes this, but does not entertain interpretations that could place his subject in a less important, more dependent role. Soly's reconstructions of the profitability of Van Schoonbeke's enterprises occasionally pass the boundary of the plausible. His long-windedness and repetitiveness also detract from the book. This remains a vastly impressive work. It offers a rare glimpse into the operating methods and economic environment of a sixteenth-century entrepreneur who combined ex-

ceptional ability with otherwise very ordinary characteristics. A short French summary is included.

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CLAUDE BRUNEEL. *La mortalité dans les campagnes: Le duché de Brabant aux XVII^e et XVIII^e siècles*. In two volumes. (Université de Louvain, Recueil de Travaux d'Histoire et de Philologie, Sixth Series, number 10.) Louvain: Bureau du Recueil, Bibliothèque de l'Université. 1977. Pp. lxiv, 777; 97. 1,800F.

This massive study exhausts the vital data of twenty-seven Belgian parishes for the period 1600–1796, draws on twenty-five more in detailing the periods of highest mortality, analyzes forty-eight published genealogies and monastic necrologies, and systematically compares its findings with others for Western Europe. It is also a compendium of methods that are explained in detail and applied: intensive source criticism (especially ages at death given in the parish registers); grouping of parishes by quality of infant death registration (the author constructs an index that reflects mortality levels of the few best registers but uses the others to exclude their random variation); study of yearly fluctuations using residuals from linear trends; detailed examination of each short period of unusually high mortality, using all available sources; study of changing seasonal variations; age-specific mortality, including a meticulous breakdown of infant mortality; calculation of life expectancy at certain ages, using model life tables; and a study of the mechanisms of the major killers (plague, dysentery, and food shortage) in light of both parish register figures and modern medical knowledge.

Claude Bruneel finds that yearly and seasonal fluctuations diminished somewhat as dysentery replaced the plague after the 1660s as the major epidemic disease, only to be challenged in the eighteenth century by less spectacular killers such as influenza, tuberculosis, and smallpox. But this transformation was far from complete, and the apparent statistical trends are upset by two outbreaks of dysentery, possibly “archaic,” in the late eighteenth century. He finds little direct correlation between high grain prices and periods of peak mortality (though here his statistical analysis could be refined considerably). Infant mortality improved slightly, that of adults not at all, but that of children aged one to ten dropped significantly. The proportion of adults in the population increased, as did overall life expectancy from birth.

Bruneel does not find these modest improvements sufficient to explain the doubling of Brabant's population in the eighteenth century, and he suggests, without further development, that fertility must be the cause. But fertility increase would have swollen the younger, not the older, part of the population unless it occurred in the first part of the eighteenth century and then stopped. Bruneel alludes to but does not summarize the fertility studies for his region. If fertility did rise, was Brabant, like neighboring Flanders, a zone of rural industry, of “protoindustrialization,” with early and fecund marriages among rural workers? Bruneel's brief look at the province's economy does not suggest this. Furthermore, some toying with possible growth and reproduction rates would be necessary to see just how much growth his modest drop in child mortality *could* explain. It is understandable, in view of his already monumental labors, that he stopped when his ecclesiastical sources did; but it would have been possible, without great additional work, at least to link his data with the published mortality statistics of the nineteenth century, when mortality did presumably fall greatly.

Bruneel's work, despite its predilection for technique and analysis rather than interpretation, implicitly contradicts Thomas McKeown's grand hypotheses that declining mortality from infectious diseases was *the* cause of growth and that the causes of this decline in the seventeenth and eighteenth centuries can be adequately deduced from study of nineteenth-century causes of death, without the need for laborious microdemographic studies. *La mortalité dans les campagnes* is the largest and most thorough *summa* yet on the collective mechanisms of death in a region of preindustrial Europe outside Scandinavia, and, aside from a couple of omissions, it is a model of method—one to be referred to, however, rather than imitated, in its luxury of procedural detail.

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ANDRÉ BOLAND. *Le procès de la révolution belge: Adolphe Bartels, 1802–1862*. (Facultés Universitaires Notre-Dame de la Paix, Bibliothèque de la Faculté de Philosophie et Lettres, number 56.) Namur: Presses Universitaires de Namur. 1977. Pp. 316.

Under the most favorable circumstances biographies prove difficult to write. Political biographies of minor characters who played secondary roles in the events of their time are even more difficult. When the biographer sees in his hero's proposals

the prefiguration of still unresolved social, political, and ethnic problems, objectivity suffers.

Adolphe Bartels, by any standard of judgment, played only a minor role in the history of Belgium in the nineteenth century. In the Belgian Revolution itself he had no role since he was in exile in France at the time. Only after the crucial days of September 1830 did he re-enter Belgium at the head of a small band of volunteers. He proceeded to Bruges intent upon securing that city's adherence to the new revolutionary government. Upon his arrival he learned that the object of his efforts had already been achieved by others. Indeed, that seemed to be the history of his life. Each of his goals were more completely fulfilled by others. Receiving no recognition for his efforts from the revolutionary government and finding himself without means to influence the political course of the revolution, he retired again to France in a self-imposed exile.

A convert to the Catholic faith, Bartels was a liberal, a democrat, and a republican. His background was that of a petit-bourgeois. As the idealist in his family, he forsook a business career for one of writing. Both his brothers enjoyed financial success. A follower of the Abbé de Lamennais, he was convinced that the new Belgian state should be a liberal democratic republic. In this objective he had no following among Catholics. To Boland, Bartels was the Catholic counterpart to de Potter, a liberal who also wanted a republic. He sees in Bartels's views a program for continuing revolution, that is, social reform.

The work is scholarly and well written. It follows traditional historical methodology. The author has exhaustively researched manuscript and printed sources in the major archival depositories of Belgium, France, and Holland. His bibliography shows that he consulted the established works on the Belgian Revolution, on Lamennais, and on de Potter. In these areas he offers no new revelations or syntheses. His secondary sources are largely the works of twentieth-century Catholic liberal writers. The biography, we learn, was many years in preparation.

For the student of the Belgian Revolution the book should be read inasmuch as it offers a Catholic, democratic, liberal perspective that is lacking in the works of Pirenne, de Moulin, van Kalken, Juste, and a host of others. To the student of contemporary Belgian affairs the book is more important, for Bartels's program of social action parallels that of the Christian Democratic Party. If Belgian historians do choose the occasion of the approaching one hundred and fiftieth anniversary of their country's independence to re-examine critically their revolution, they might well test ac-

cepted positions by methods other than the traditional ones. Myths exist in all histories.

JOHN W. ROONEY, JR.
Marquette University

MICHAEL F. METCALF. *Russia, England and Swedish Party Politics, 1762-1766: The Interplay between Great Power Diplomacy and Domestic Politics during Sweden's Age of Liberty*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. viii, 278. \$35.00.

Michael F. Metcalf's book is actually a doctoral dissertation prepared and written at the University of Stockholm within the context of a graduate/postgraduate research project entitled "The Development of a Party System: Parties, Parliamentary Practice and Parliamentary Ideas in Sweden, 1680-1772." As such it contains both the strengths, academically speaking, and weaknesses, from a stylistic point of view, of a regular dissertation.

The author has been very diligent and has invested a great amount of work into this research project, which is rather limited in scope. Metcalf has done research not only in the archives of Sweden and England, as one would normally expect, but also considerable additional work in the Danish, French, East German, Dutch, and Soviet archives (which are normally closed to foreigners). This fact alone adds additional value to this book. Metcalf has also used all available printed sources and secondary works in English, Swedish, French, German, and Russian. Metcalf's footnoting is meticulous and copious, and his bibliography and indexing are excellent. The author adjusts all the dates to the Gregorian calendar and spells all names, with the exception of certain monarchs, in the native language, which this reviewer finds commendable.

The so-called Age of Liberty (1718-71) is a rather obscure but interesting period in Swedish history. Its principal characteristics are the general decline of Sweden as a European power, serious economic travail, the absence of strong rulers, a constitutional experiment with the emergence of a two-party system ("Caps" and "Hats"), and foreign interference in the form of subsidies, bribes, and pressures. The differences between the Caps and Hats were no clearer than the differences between Republicans and Democrats in the modern U.S., but, generally speaking, the Hats were more sensitive to the problems of national prestige and defense, favored strong protective economic policy, and tended to be pro-French, while the Caps were more realistic and pacifistic in their foreign policy and tended to be more or less pro-

Russian. The Hats were vaguely identified with the upper aristocracy and commercial groups, while the Caps were mostly lower aristocrats, clergymen, farmers, and members of the court.

Metcalf chooses a very limited period (1762–66), associated more or less with the advent of Catherine II to the throne of Russia and vigorous Russian foreign policy. The Hats were dominant on the Swedish scene from 1739 to 1765, while the Caps came to power in 1765. The author attempts to determine the amount of influence that foreign courts mustered during this period of transition by way of large sums of money to support one or the other of the competing Swedish political parties. He takes issue with some recent historians who play down the significance of foreign powers and foreign gold, and reaches the conclusion, to no one's surprise, that foreign money used to finance Swedish parties and politicians indeed played some role in shaping Swedish foreign policy. Without Russian and British subsidies the Caps would not have been in a position to compete with the French-supported Hats when it came to maintaining a plurality in the House of Nobles.

Considering the sad fact that, with a few mostly outdated exceptions, only general surveys of Swedish history are available to English readers, this book is a welcome addition to our rather meager collection of detailed studies in Swedish history.

EDGAR ANDERSON
San Jose State University

PETER CLAUS HARTMANN. *Geld als Instrument europäischer Mächtpolitik im Zeitalter des Merkantilismus: Studien zu den finanziellen und politischen Beziehungen der Wittelsbacher Territorien Kurbayern, Kurpfalz und Kurköln mit Frankreich und dem Kaiser von 1715 bis 1740.* (Studien zur Bayerischen Verfassungs- und Sozialgeschichte, number 8.) Munich: Kommission für Bayerische Landesgeschichte. 1978. Pp. xi, 284.

The author of this book, a young German historian born in 1940, is a staff-member of the Deutsches Historisches Institut in Paris and a specialist in modern and recent European history. He has written short pieces on the Treaty of Versailles, the German youth movement and National Socialist education, and France under German occupation in 1941. He is also the compiler of a useful guide to Paris archives and libraries that harbor materials for the study of the nineteenth and twentieth centuries.

In the work under review, a *Habilitationsschrift*, Peter Claus Hartmann takes up anew themes he developed in his 1967 Munich dissertation, "Die

Finanz- und Subsidienpolitik des Kurfürsten Max Emanuel von Bayern und der kurbayerische Gesandte in Paris, Comte D'Albert-Fürst Grimbergen," and in eight articles published between 1968 and 1973.

The author addresses the following topics: the economic and political structure of the Wittelsbach territories, Bavaria, Cologne, and the Palatinate, in the early eighteenth century; the foreign policy of Bavaria and Cologne from the end of the War of the Spanish Succession to 1724; the policy of the Wittelsbach principalities in 1724–26, the period of the *renversement des alliances* inaugurated by the Treaty of Vienna in 1725; the relations of the Wittelsbach states with France, 1726–33; and the relations of those states with France and the emperor between 1733 and the death of Karl VI in 1740.

Hartmann's thesis breaks down into four parts and a corollary. The Wittelsbach states lacked the economic and military strength to be truly sovereign, even by eighteenth-century standards; they were overwhelmingly dependent on French (or Austrian) subsidies. These states were unable, in view of their inherent weakness and Austrian opposition, to create a third force in Germany between 1724 and 1726. They were, moreover, incapable of presenting an effective alternative to the election of another Habsburg emperor in 1740. In the long term, the failure of the Wittelsbachs to create a third force opened the way to the baneful rise of Prussia after 1740. And the French policy of subsidies not only failed to produce results in 1740 but also deranged the Bourbon fiscal structure and thus contributed to the outbreak of revolution in 1789.

Using a wealth of archival and secondary sources, the author argues his thesis in elegant fashion. There are interesting details on the mechanics of the payment of the ultimately unproductive subsidies. The book includes a brief annex of diplomatic documents from the period 1728–40, a twelve-page bibliography, and indexes of persons, places, and things. There are several simple (and therefore useful) tables and graphs on comparative national strengths and methods of payment and a somewhat smudgy but necessary map of the scattered Wittelsbach lands. The map, buried in the middle of the book, is not handy for immediate reference. Indeed, there is no table of contents for the graphic materials. The work is well written and beautifully produced, with few misprints of the type that tend to creep into polyglot footnotes and bibliographies. It is, in sum, a valuable contribution to the diplomatic-financial history of the age of absolutism and "mercantilism" and a worthy addition to the literature in the

Rankean tradition of the history of relations among states, as modified by the structuralism of Bloch, Febvre, Braudel, and their disciples.

RICHARD H. THOMPSON
Bloomington, Indiana

NOTKER HAMMERSTEIN. *Aufklärung und katholisches Reich: Untersuchungen zur Universitätsreform und Politik katholischer Territorien des Heiligen Römischen Reichs deutscher Nation im 18. Jahrhundert*. (Historische Forschungen, number 12.) Berlin: Duncker and Humblot. 1977. Pp. 276. DM 78.

Notker Hammerstein's new book sustains the thesis of his *Jus und Historie* (1972), in which he treated the reform of the German Protestant universities at the beginning of the eighteenth century and their adaptations to the world of Aufklärung and absolutism. Hammerstein attributed that reform to the example of Halle and Thomasius, the modernization of the law curriculum, and the rise of law to replace theology as the dominant faculty in the university. From this standard interpretation he drew more controversial conclusions. Hammerstein portrayed Göttingen as a mere continuation of the Halle pattern and denied it any fundamentally innovative role; he minimized the impact of Christian Wolff; and he seemed to dismiss by silence the importance of Pietism or rationalism as agents of important curricular reform.

Having traced in *Jus und Historie* the dissemination of the Halle reforms through the Protestant universities, Hammerstein assesses in the volume under review the extent to which German-speaking Catholic universities subsequently adopted these reforms. His interpretation holds a middle ground between that of Protestant publicists, who formerly denounced the Catholic universities as centers of obscurantism hostile to the Enlightenment, and more recent revisionists, who dismiss these condemnations as groundless bigotry. Only the hapless university at Cologne remained wholly untouched by Protestant innovations, he notes, while Ingolstadt, Mainz, Würzburg, and Bamberg belatedly remodeled their law curriculum along Protestant lines. Hammerstein portrays the Jesuits as the chief obstacles everywhere to reform. The agents of reform, on the other hand, were legal scholars, such as Johann Ickstatt, Johann Horix, and Franz Schrötter, and their absolutist patrons, the Schönborn family at Mainz, Max Joseph in Bavaria, and Maria Theresa.

Catholics did not merely adopt Protestant innovations in toto. Christian Wolff had far greater impact on Catholic than Protestant universities. The disciplines of canon law, natural law, *Jus publicum universale*, and forms of erudite history

were marginal disciplines in the Protestant universities but fundamental innovations for the Catholic ones. Above all, the Catholic world failed to adopt the ideals of *Lehr-* and *Lernfreiheit* espoused at Halle, and it rarely extended to its professors the freedom from censorship taken for granted at Göttingen. Especially in Austria, Catholic university reform was Aufklärung imposed from above by princes mobilizing the universities to their own needs and subduing theological opposition in the process.

Although Hammerstein's new work marks a valuable contribution to university historiography, it shares many of the liabilities of *Jus und Historie*, including that of largely ignoring innovations emanating from outside the law faculties. His is a highly specialized and intellectualized concept of reform, to be traced through the textual analysis of reform tracts and measured through the creation of new nominal professorships. We hear little about the realities of enrollment figures, financing, or the demand for graduates. And the book suffers from long-windedness, a defect that casual readers will find partially offset by an excellent summary chapter.

R. STEVEN TURNER
University of New Brunswick

DETLEF K. MÜLLER. *Sozialstruktur und Schulsystem: Aspekte zum Strukturwandel des Schulwesens im 19. Jahrhundert*. (Studien zum Wandel von Gesellschaft und Bildung im Neunzehnten Jahrhundert, number 7.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 849. DM 140.

This is the most massive study of the nineteenth-century German school system to appear in generations, and it is a most ambitious analysis of the relationship between German social structure and school development. It is an admirable product of years of diligent research, careful statistical analysis, and application of social science concepts to German educational history. It is a bibliographical and statistical mine for students of the Prussian school system: tables, notes, and lists of sources take up over 500 out of 849 pages.

In fact this work is really two volumes in one: the first, written in prose, traces the rise of the Prussian school system from the late eighteenth to the early twentieth century; the second, written in mathematical formulas and time-series tables, analyzes such variables as male school population, educational "chances," intergenerational social mobility, finances, and other factors pertinent to schooling and social structure. Both parts contain informative and pioneering work. Yet both have limitations that reduce the usefulness of the book.

The limitations of the statistical portion derive from Detlef K. Müller's almost exclusive reliance on evidence from the city of Berlin in the period 1828-1914. One can hardly fault the exhaustiveness of the analysis—in some respects, too much information is reproduced here—or fail to sympathize with the author's dilemma in trying to find continuous useful figures. But one can equally well doubt the typicality of a school system catering to the population of an immense city disproportionately packed with both industrial workers and government functionaries.

The first part of the book does not, however, depend directly on the second; instead, it describes the evolution of the Prussian school system as a whole. This in itself is a worthy and useful service. The limitation of this part lies in the themes that hold the analysis together, all of which can be traced back to the author's implicit assumption that schools ought to be the major vehicles for social mobility. Müller doggedly proceeds through his first twelve chapters to demonstrate that this role was not filled by the developing German school system and that it even reduced the chances for such mobility as it grew and diversified. This argument amounts to a backward projection of current criticisms of the "undemocratic" West German school system and its roots in the nineteenth century. But it begs at least two serious questions: what objective needs beyond a desire for social stability motivated the changes in schools in an admittedly "undemocratic" society? What is a "democratic" school system?

Müller serves us well by analyzing and quoting extensively from the writings of school reformers, administrators, teachers, and pedagogical theorists. But he displays a regrettable penchant for dismissing discussions of educational goals as "veiled social interest politics." It is laudable to redress the suppression of this factor in so much of the older literature on German educational history. But the redressing is done with so much overkill that other valid factors in educational change are not duly considered. It is this "aspect," mentioned in the subtitle, that chiefly limits the scope of this impressive contribution to social and educational history.

CHARLES E. MCCLELLAND
University of New Mexico

JOACHIM PASCHEN. *Demokratische Vereine und preussischer Staat. Entwicklung und Unterdrückung der demokratischen Bewegung während der Revolution von 1848/49.* (Studien zur Modernen Geschichte, number 22.) Munich: R. Oldenbourg Verlag. 1977. Pp. 182. DM 56.

The subject of this study consists of the radical political associations in Prussia during the 1848-49 period and constitutes volume twenty-two of the series "Studien zur modernen Geschichte," edited by Fritz Fischer, Klaus-Detlev Grothusen, and Günter Moltmann. The author, Joachim Paschen, has undertaken a thorough study of the relevant archives, not only in the German Federal Republic but also in the German Democratic Republic. He draws both on the minutes of the associations and on the reports written by the authorities. The political associations were certainly an important factor in the period under review. The book deals with those associations that called themselves "demokratisch" (or something similar) rather than "konstitutionell," that is to say radical rather than moderate liberal. Although the distinction between the two groups was often somewhat blurred, it had acquired a certain validity even before the revolutions of 1848-49. Some hitherto accepted generalizations stand up against the detailed information. The moderate liberals were mainly the party of the middle class and the upper middle class, whereas the radicals had their principal base of support among the lower strata. The moderate liberal program emphasized political and constitutional issues, whereas the radicals put social questions in the foreground. But the author shows that one should not necessarily equate the "democrats" with republicans; not all of them were committed to a republic. He produces evidence, too, that collaboration between the parliamentary and the extraparlimentary opposition was not as smooth and cordial as has sometimes been thought. Often the two groups were jealous of each other. Indeed, a close examination of the Left reveals that it was far less united than appeared from the outside. There were great difficulties in securing general agreement on a detailed program, for example, concerning social and economic questions. Nothing would be more erroneous than to perceive the radicals of 1848 as supporters of a welfare state.

Curiously enough, the Left and the authorities entertained the same misconception: both exaggerated the strength of radicalism. Many of the measures that the government took against the Left appear in retrospect as excessive and unnecessary. It is unfortunate that the conservatives often had no other weapon against radical ideas than censorship, arrests, and repression. Surely the authorities sometimes had a good case against the radicals, which they allowed to go by default. They did have a duty to maintain law and order against those forces of the Left that threatened it. The author is good on the microstory and stimulating on macrohistorical development. But he is unable to spell out all his assumptions in a short tract. It is unfortunate that he does not allow the

conservatives to argue their case, instead of writing them off as "reactionaries."

FRANK EYCK
University of Calgary

RICHARD J. EVANS, editor. *Society and Politics in Wilhelmine Germany*. New York: Barnes and Noble. 1978. Pp. 305. \$19.50.

In his introduction to this volume of essays by ten young British historians, Richard J. Evans explains that their purpose is to expand the study of Wilhelmine society into hitherto neglected channels. While acknowledging a debt to the middle generation of German historians, Evans claims that they have concentrated too narrowly on the manipulative Prussian elite and have disregarded the Wilhelmine period's liberal, progressive trends, its regional diversity, its grass roots movements, and the role of the Center Party. By focusing too much on the antecedents of National Socialism, they have overlooked the historical continuity between the Wilhelmine and contemporary periods. The contributors to this volume propose to remedy this situation.

How well do they succeed? The four essays dealing directly or indirectly with the Center achieve their purpose totally. Three treat that party's ambiguous role in the liberalization and democratization of German politics; two are set in Bavaria; one demonstrates the growth of grass roots peasants' organizations. The discussion of the Bavarian Peasant Leagues and their influence on the Center's agricultural program raises the wider question of the political mobilization, not only of the peasants, but of the *Mittelstand*. Contrary to the view that these groups were politically integrated by a governmental *Sammlungspolitik*, Geoff Eley suggests that they had their own independent demands, which they inflicted upon the elitist leaders of political and economic interest organizations. In his view, further study of the rising Wilhelmine *Mittelstand* would increase our understanding of events in the 1930s. As Arno Mayer has recently argued, such research would also deepen our insight into contemporary society. One last article on middle-class organizations—Evans's summary of German feminist history—acts as an intellectual *hors d'oeuvre*; it whets the appetite for his recent two books on the same subject.

Two of the three final essays on the working class and the Social Democrats are chiefly valuable for raising questions. To what degree was the labor movement fragmented and spontaneous rather than well organized and disciplined? How did this lack of coherence affect work actions and labor politics? What was the extent, motivation, and

composition of working-class radicalism both before and after 1918? Lastly, Alex Hall's exposure of the Socialists' ambivalence toward their own youth movement during the Wilhelmine period aptly demonstrates the long history of the current difficulties between that party and many of its youthful adherents.

This book is a handy package for introducing this group of British historians to a public wider than that which assiduously scans the scholarly journals. Although the authors rely on a large fund of archival sources, many of the essays suffer from a lack of organizational and conceptual clarity. Some parallel the contributors' previous books and articles too closely. Nevertheless, the group has rendered a service to all German historians by explicitly devoting an entire volume to increasing the scope of inquiry into Wilhelmine society and politics.

BEVERLY HECKART
Central Washington University

HARTMUT KÖNIG. *Bismarck als Reichskanzler: Seine Beurteilung in der sowjetischen und der DDR-Geschichtsschreibung*. (Dissertationen zur Neueren Geschichte, number 3.) Cologne: Böhlau Verlag. 1978. Pp. ix, 294. DM 45.

According to Hartmut König Soviet and East German historiography have not been appreciated properly in the West. They are not, he claims, as sterile or as tightly handcuffed to party ideology and politics as many Westerners would claim. König argues that Communist historiography has, on the contrary, gone beyond the emphasis on diplomacy and personality, which he sees as the hallmark of West German historiography. Marxist methodology in the Soviet Union and East Germany fruitfully stresses the primacy of socioeconomic factors in historical development; it recognizes certain historical laws and yet seeks to integrate the great historical personality into the historical process. The author seeks to make his case for the richness of Marxist historiography using Bismarck, the imperial chancellor, as the focus for his discussion. Bismarck is merely a convenient specimen for this analysis of historical writing in the Soviet Union and East Germany. The reader learns nothing new about the Iron Chancellor, but he does become acquainted with a variety of Marxist interpretations of Bismarck's policies and personality. The title of the book is thus somewhat misleading.

The major part of the book consists of a discussion of Bismarck-related issues taken up by historians in the USSR and East Germany. These issues include German unification and Bismarck's

role in that development, his foreign and domestic policies, and his significance for German political development. König then investigates—too cursorily—the relationship between the writing of history and political developments in the Soviet Union. (He fails to undertake a similar analysis of East German historiography.) Not surprisingly, he finds, for example, that in the Stalin era Bismarck's personality received more attention and appreciation than in either the pre- or post-Stalin eras. He concludes that Soviet Bismarck historiography has not remained within the limits established by Marx, Engels, and Lenin. König views it as a dynamic process, characterized by "dialectical progress in understanding the period 1871–1890" (p. 250).

König's book (his dissertation) is well organized and well written. His desire to facilitate greater appreciation of the flexibility, independence, and fruitfulness of Marxist historical writing is laudable. The presentation of his case, however, leaves something to be desired. König's central problem is partiality. Instead of judiciously analyzing the strengths and weaknesses of Marxist historiography, he presents the reader with an unsubstantiated argument. He fails to criticize questionable assertions made by Soviet and East German historians. He does not comment on the claims that there was a viable alternative to Prussian unification of Germany or that Bismarck sought destruction of Germany's rivals in order to establish German hegemony in Europe. Presumably, the author agrees with these as well as other debatable assertions. On a number of occasions, however, he does suggest that Soviet historians have not advanced beyond the perceptive comments made by Marx, Engels, and Lenin. Thus König leaves the reader to ponder what it is that Soviet historiography has done to extend our understanding of Bismarck. König suggests, however, that East German studies have been more fruitful than Soviet ones: their authors have been more faithful to the Marxist-Leninist creed (less emphasis on diplomacy and personality) and more thorough in their investigation of sources than the Soviets. But these ideology-burdened studies do not receive sufficient attention, nor does the author explain how they have compensated for the supposed shortcomings of West German historiography.

König's criticism of West German historians goes too far. Admittedly, not all have jumped on the socioeconomic bandwagon. But such pluralism is good and distinguishes West German historiography from its counterparts in the USSR and East Germany. No one can deny the innovative spirit and the dynamic, if not aggressive, controversies that have characterized the West German historical profession since the early 1960s. If

anything, East European historiography has been unimaginative and has been forced to respond (see the essay by T. P. Koops in *Die Funktion der Geschichte in unserer Zeit*) to West German methodological advances. König is too much a missionary and not enough an impartial observer.

The book is not without merit. The author brings together some interesting comments concerning Bismarck. He familiarizes the reader with some Soviet historians, their works, and their shifting perspectives (as their political environment changes). Above all, König encourages the reader to view Marxist historiography with an open mind.

JUERGEN DOERR
St. Thomas University

LOUIS L. SNYDER. *Roots of German Nationalism*. Bloomington: Indiana University Press. 1978. Pp. x, 309. \$17.50.

This book illustrates well the basic work of Louis L. Snyder, who has, over a generation, written and edited many books on nationalism and race, particularly of the German varieties, and has become a renowned authority. The book contains thirteen essays, six heretofore unpublished and seven published previously in scholarly journals but here somewhat revised in order to relate them more closely to German nationalism. The essays demonstrate the wide reading and research behind Snyder's books.

In his own words Snyder's purpose in this volume is to "throw light on various aspects of German nationalism by combining some of my articles and lectures with additional material written especially for this study" (p. viii). The essays cover the "ism" chronologically from the early nineteenth century to the present. Snyder's approach is "pluralistic"; his interpretations are largely in the tradition of Carlton Hayes and Hans Kohn, both of whom, especially the latter, he admires. Snyder writes chiefly on the scholarly level of lectures for advanced undergraduate and graduate students. If much of the content does not appear to be new, this is perhaps because Snyder and others, particularly after the rise of Hitler, have done so much work on German nationalism that the results are well known. Snyder's new book, however, does add here and there to what is generally known.

The essays cover many of the varieties of nationalism (especially German): the economic nationalism of Friedrich List and the *Zollverein* and the cultural nationalism of the Grimm brothers; the national political consolidation of Germany through Bismarck, an episode in German expansion—the penetration of South West Africa (1880–

85)—and the ridiculous controversy over German prohibition of American pork imports (1878–91); Bismarck's "German" authoritarianism as seen in the denial of his son's request to marry the Princess Carolath and in the refusal to accept an American House of Representatives resolution lauding Bismarck's enemy Edward Lasker; the popular nationalistic myths perpetuated by the medals of Karl Götze (here illustrated); the horrible "detritus" of Hitler's nationalism; a tribute to the work of Hans Kohn; the relationship of the Lorenz, Ardway, and Morris (Snyder's term LAM) guesses on the aggressive nature of people to Germans; and some reflections on German nationalism after 1945.

Perhaps the best chapter is on the Grimm brothers, the most impassioned chapters are those on Hitler, and the least convincing one (through no fault of Snyder) is on man's aggressiveness. The chapter on the Grimms shows (on the basis of the research of Heinz Rölleke) that their superb *Märchen*, including "The Sleeping Beauty," often came from Charles Perrault of France and through French Huguenots. This reviewer's interest was greatest in the section devoted to the question: was Bismarck a German nationalist or a Prussian-Junker-statist? Over twenty years ago (April 1955) Otto Pflanze, then at the University of Massachusetts, published a judicious article in this journal that surveyed the state of the question at that time. The judgments of scholars, among them Friedrich Meinecke, Hans Rothfels, and Gerhard Ritter, were then divided; and they still are. Snyder calls Bismarck an "apostle of nationalism," but was he until 1866 chiefly a Prussian-Junker-monarchist-statist who thereafter became a German nationalist or at least used national feeling to unify Germany, although he did not believe in uniting all Germans and was not an absurd socialist?

This reviewer so often agrees with Snyder that he finds it difficult to be critical. But did not German nationalism take root earlier than the nineteenth century? Can one speak of what the German people believed? Is not the article "the" too often used when "a" or no article would be more accurate? Snyder's prose is clear but does he need phrases like "chain reaction" or "wheels in motion"? Snyder detests Hitler and the Nazis (as does this reviewer), but does the description of "this narcissistic, sadomasochistic and necrophilic Fuehrer" (p. 277) sound like a sound historical judgment? These few caveats aside, one will profit from Snyder's discussion of the issues he so provocatively raises; and certainly his *Roots* will be cited again and again both on nationalism in general and on the German varieties.

BOYD C. SHAFER
University of Arizona

MANFRED RAUH. *Die Parlamentarisierung des Deutschen Reiches*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 60.) Düsseldorf: Droste Verlag. 1977. Pp. 533. DM 78.

In his second study of Germany's parliamentarization, focusing upon 1909–14, Rauh uses many novel sources from provincial archives. But the study is almost a counterargument to the findings—repeatedly termed unfounded assertions (pp. 9–14, 289–91)—of the "critical" school of German history (Wehler, Berghahn). Rauh challenges the view that the structural deficiencies of the Kaiserreich made reform impossible, as well as the concept of the primacy of internal affairs. In presenting his own ideas, Rauh claims to have developed a more sophisticated type of historical analysis than any social-structural school offers. He seeks to prove two related theses. First, the Kaiserreich was undergoing a "silent parliamentarization" in the years before the First World War. Second, the events that brought Germany a parliamentary system in 1918 were not advantageous to the "ripening of democracy" in Germany. While most historians of modern Germany would agree with the latter assertion, given the hindsight of the Weimar Republic, few would accept Rauh's assessment of the revolution of 1918 as unnecessary for Germany's parliamentarization, even in the form of a constitutional monarchy. Especially suspect is Rauh's implication that the conservative elites were prepared to accept such a parliamentarization.

Rauh presents the thesis that Germany underwent a "silent parliamentarization" through highly selected events. For example, he tries to show how the hegemony of Prussia was dissolved within the Bundesrat and replaced by the preeminence of the Reich chancellor and national cabinet. With test cases regarding taxation powers (*Schiffahrtsabgabe* and *Wertzuwachsststeuer*) he attempts to illustrate the Reich's and the parliament's growth in power. The argument depends upon the extent to which the political parties were consulted in formulating legislation and how the responsibility of the chancellor increased, almost to a public accountability. In order to maintain that these changes brought Germany to the "edge of a parliamentary system," Rauh has to perform some verbal acrobatics. He becomes almost an apologist when discussing the meaning of a non-confidence vote by parliament, the constitutional system for Alsace-Lorraine, or the lack of change in the three-class Prussian voting system—all those issues on which much more was said than done. Undoubtedly, Rauh has added a novel dimension to an understanding of recent German history, namely, by demonstrating that the Bis-

marckian system as focused in the Bundesrat had disappeared by 1909. But he has not shown clearly what he claims was on the other side of the coin: the weakened Bundesrat was on the way to being incorporated into a democratic system (p. 144). He has not paid attention to the possibility that the Prussian elites found in an increasingly centralized national state (for that is primarily what he has shown well) their best protection against social change, a perspective that would support Rauh's opponents in the primacy-of-internal-politics school. His own focus upon the process of politics can hardly serve as a means to counter analyses that set the political conflicts in the larger setting of classes, economic interests, and social relationships. Rauh offers a corrective to an overly deterministic approach, but his account does not warrant the claims made in the lengthy preface and footnotes.

D. K. BUSE
Laurentian University

HSI-HUEY LIANG. *The Sino-German Connection: Alexander von Falkenhausen between China and Germany, 1900-1941*. (Van Gorcum's Historical Library, number 94.) Assen, The Netherlands: Van Gorcum. 1978. Pp. xv, 229. f 39.50.

Although the process of modernization is only an underlying theme of *The Sino-German Connection: Alexander von Falkenhausen between China and Germany, 1900-1941*, the social groups that formed the elements of Sino-German relations during the interwar years were inextricably involved in change. Written as social history, this is a study of intercultural relations that provides a framework within which the author shapes his controversial thesis of a Sino-German "partnership . . . that was never consummated" (p. xi) and that "produced no results of lasting consequence" (p. 172).

A major strength of the work lies in new sources. The interviews by Hsi-Huey Liang, a professor of history at Vassar College, impart an element of human recollection that adds an important dimension. His detailed view of the Chinese community in Germany is perceptive, ranging from the lower-class Chinese in Berlin ghettos to the student movement, with its undertones of modernization through technological education. On the basis of Chinese Foreign Office materials, interviews, and personal memorabilia, Liang explores the role of the Chinese legation in nurturing the Sino-German connection. After 1928, the office assumed a more dynamic stance, primarily through the creation of a "Commerce Department," which served as a cover for the recruitment of military advisers and for contacts with German business interests.

Recently opened papers of Alexander von Falkenhausen, to which Liang had access, unveil new facets of the general's position vis-à-vis China and Germany. In addition to Falkenhausen's recommendations to Chiang Kai-shek relative to strategy, tactics, and reorganization of the Chinese forces, the papers expose the question of Reichswehr control of appointments to the advisory group as a condition of continued support for China after 1934. Liang confirms this pressure by citing materials recently attained by the military archives in Freiburg.

The author is ill served by publication "grem-lins," such as transposition of the title between table of contents and text, and numerous typographical errors. Substantive problems of varying severity also appear, such as the questionable assertion that Chinese nationalism was passive in the 1930s. Liang's evidence to show that the Sian incident of December 1936 had positive effects on China's image in German circles ignores the Nazi decision to conclude the Anti-Comintern Pact with Japan in November. Furthermore, the astonishing statement that execution of hostages was a "legally recognized practice of warfare" (p. 177) requires substantiation from international law.

Although the book presents no particularly new interpretations, it redresses the balance between the material and cultural motivations of the participants. The study adds both depth and color to events that intrigue social and military historians, as well as specialists in modernization theory. Moreover, the intercultural relationships that were the essence of the old Sino-German connection contain living suggestions for the future. Although economic and military relations frayed and broke, cultural ones did not; the author's list of interviewees includes German-educated Chinese who attained (or still hold) positions of authority in Taiwan. How do we measure Liang's "lasting consequences" in an era of interdependence of nations?

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HERMANN LUPPE. *Mein Leben*. In cooperation with MELLA HEINSEN-LUPPE. (Quellen zur Geschichte und Kultur der Stadt Nürnberg, number 10.) Nürnberg: Selbstverlag des Stadtrats zu Nürnberg. 1977. Pp. xiv, 371. DM 33.

HERMANN HANSCH. *Oberbürgermeister Hermann Luppe: Nürnberger Kommunalpolitik in der Weimarer Republik*. (Nürnberger Forschungen, number 21.) Nürnberg: Selbstverlag des Vereins für Geschichte der Stadt Nürnberg. 1977. Pp. xii, 429. DM 36.

If Gustav Stresemann's cryptic diary comment that "along with the big industrialists, the mayors of contemporary Germany are in reality the present day kings" has any validity, then the appearance of two books on Nuremberg's embattled Weimar mayor, Hermann Luppe—his memoirs and Hermann Hanschel's thoroughly researched and sympathetic biography—should provide scholars with valuable material and new perspectives into the Weimar era. Few will be disappointed. While Luppe's memoirs, written during the years of internal exile under the Nazis, constitute, as Hanschel notes, a first-rate primary source, both studies provide detailed insights into the often neglected world of real life on the local level. Luppe describes in clear and candid prose how the revolution, inflation, threats of separatism and depression, and the incipient Nazi movement affected his city and its people; he also reveals how an ambitious, competent administrator, with a passion for democracy, tried to educate his constituents through service and word to make the new government work. From concern for community cultivation of the fine arts, through the difficult problems of education and welfare, to the preservation of Nuremberg's *Altstadt* (an architectural jewel) from the threats and demands of technological modernization, Luppe responded optimistically to a gamut of challenges. At the same time he hoped to transcend his provincial setting in order to promote the fortunes of democracy and the republic on a national level. Here his success was, at best, limited. Luppe's public life does, in fact, offer a case study of the tragedy of both the German Democratic Party and German democracy in general. Unlike a majority of his fellow party members, he was a *Herzensrepublikaner*, with strong antimonarchist, antimilitarist, and protosocialist sentiments dating back to his Kiel youth. An early correct assessment of his talents blended with an ethically grounded commitment to the improvement of life for the common people that thrust him headlong into a career in municipal administration; throughout his life he believed his choice of career had fitted him perfectly. A strong dose of idealism (though he considered himself a practical man rather than a theoretician) also steered him into the ideological currents of social democracy, though he rejected emphatically any attachment to Marx or to any form of planned economy.

Both volumes reveal Luppe as a consummate, intelligent local administrator, able to solve problems while serving as a spokesman for his democratic Weltanschauung. They also chronicle his relative impotence as a positive influence in his party. He was respected and even admired, but the results of his efforts seem to be meager. In my

judgment, it is on this issue that Hanschel might have been more critical of his subject. Did Luppe, in fact, because of a certain strain of ideological rigidity, or ambition, or both, contribute his share to the basic flaw of the democrats—the failure to create a strong, unified, broad-gauged *Sammelpartei* structure, capable of appealing to a wider range of middle-class Germans? Is there not some irony in the fact that precisely those political leaders of Luppe's persuasion failed to understand and implement in their own party organization the principles of compromise and inclusiveness in order to combat the tendencies toward splintering and radicalization? Perhaps Luppe's flirtation with social democracy (he contemplated joining the party on several occasions), his defense of proportional representation, and advocacy of Preuss's centralism unduly lessened his influence with his fellow democrats. He was unable to fulfill a role for which Hanschel considers him suitable—as a link between the right and left wings of the party, not only because the party "wander[ed] off to the right," but because he wandered off to the Left.

If one may take Luppe gently to task for what else he might have done for the republic, one can hardly fault him for his response to National Socialism. Virtually from beginning to end, he had the party, its goals, and methods pegged correctly. His patient and courageous behavior over the years of Streicher's slander and attempted character assassination, the court battles, and vilification by the press all elicit admiration. In the end, however, and notwithstanding his attempts to combat it, his political base gradually eroded as *Nürnberger*, like other Germans, grasped at radical promises to placate a growing sense of despair.

Finally, as Hanschel points out, Luppe's memoirs bear a powerful witness to what a perceptive observer, with only public sources of information, could know about the real activities of the Nazi regime. He was not fooled, and while he could take some comfort in the efforts he had made, he was saddened by the evidence of the Weimar failure right up to the time when his life ended in mid-sentence during the 1945 Hamburg bombing raid.

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MICHAEL WOLFFSOHN. *Industrie und Handwerk im Konflikt mit staatlicher Wirtschaftspolitik? Studien zur Politik der Arbeitsbeschaffung in Deutschland, 1930–1934.* (Schriften zur Wirtschafts- und Sozialgeschichte, number 30.) Berlin: Duncker and Humblot. 1977. Pp. 504. DM 116.

In view of the current discussion concerning the relationship between industry and politics, Mi-

chael Wolffsohn's conclusions merit attention. They are the result of a detailed but often pedantic analysis of the work creation programs, their place in the political arena of 1930 to 1933, and their position with regard to the priorities, actions, and reactions of big business, middle-sized enterprises, artisans, and their representative organizations. Neither monopoly groups, nor left nor right Keynesians dominated the decision-making process. Rather, a host of diffuse arguments, counter-arguments, and often contradictory and changing statements contributed to an amorphous discussion, while the work creation programs themselves were being formulated by politicians. This is an important fact in view of the tendency to see clear-cut policies and plans behind the actions of industries and industrialists. And yet, one can overemphasize a lack of direction and influence. Although Wolffsohn is convincing when he shows that heavy industry and the *Reichsverband der deutschen Industrie* (RdI) began to favor certain work creation policies in August 1932, he only dimly realizes that work creation programs never played a central role at that time or afterwards. There were more important issues at stake, like the so-called social costs of production, to which work creation was allied. One can find many reasons why industrialists preferred to downplay work creation programs; ignorance and economic orthodoxy were among them. The major barrier, however, seems to have lain in the notorious fear of German industrialists that work creation programs—especially those of the so-called inflationary variety, such as the Wagemann-Plan—could develop into full-fledged plans for an overall economic policy of the government. Not even Papen or Hitler could allay this suspicion entirely, although the latter's new and stable order certainly provided maneuverability in 1933.

Exactly what happened in that decisive year is not quite clear from Wolffsohn's account. He is correct in stressing that the work creation programs in 1933 did not include expenditures for rearmament (although, upon closer inspection, the long arm of the military would have become apparent in a variety of civil projects). On the other hand he has simply stopped short of the whole question of financing rearmament, which was the decisive issue in the first half of 1933, and because of this he gives an incomplete—and in this case a rather misleading—picture not only of the general situation but also of the work creation programs and their function. It is indeed true that work creation remained largely civil and was geared toward "social" and immediate economic needs. But this was only possible because the much maligned social costs of production had been lowered forcibly and because the financing of rearmament

had been arranged separately by Schacht—this, incidentally, exactly according to the methods recommended by the RdI in August 1932. Thus, work creation in 1933 became a civil, temporary, and marginal issue. It was stopped at the very moment when rearmament could go ahead in December 1933.

Under these circumstances one might question whether industrial groups were as powerless as Wolffsohn paints them when he claims that they bickered with even the most favorable programs of Papen and shows that they never delivered the blueprints for any kind of political decision. It seems that exactly this successful withdrawal of the work creation programs onto the fringes of economic activity points toward the influence of industrial groups. Such details illustrate the major weaknesses of Wolffsohn's study. Despite a scrutinizing analysis of the attitudes of industrialists vis-à-vis work creation, he hardly ever manages or even cares to see the topic within the overall context of industrial, economic, and bureaucratic interests and policies. He takes the importance of work creation programs for granted, instead of questioning exactly this point.

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DÖRTE WINKLER. *Frauenarbeit im "Dritten Reich."* (Historische Perspektiven, number 9.) Hamburg: Hoffmann and Campe Verlag. 1977. Pp. 252. DM 36.

One branch of the recent reinvigoration of studies on National Socialism springs from the women's movement and concentrates on women in the Nazi period: a lively oral history project is going on at the Free University of Berlin; a recent conference in Berlin brought together over eighty women from all parts of West Germany to discuss their research on this subject; and scholars from several countries are exploring party and state archives for a deeper understanding of the apparently anomalous position of the Nazi party on women. Now, to the works of Jill Stephenson, Ursula von Gersdorff, Tim Mason, Leila Rupp, Claudia Koonz, and Annemarie Tröger, we can add that of Dörte Winkler, who tells us all we ever wanted to know—and more—about Nazi policies to mobilize women for war work.

Much of her story is familiar: the initial thrust against married women in the labor force, continuing depression policies of the Weimar Republic; the about-face when defense production ended unemployment and a labor shortage surfaced; the insatiable labor needs of the war economy; and the

work evasions of middle- and upper-class women. But Winkler also offers something new—a detailed study of the behind-the-scenes conflict about appropriate policies for these problems. Analyzing the clashing needs of state, party, and economy, she concludes that Hitler himself was the main obstacle to women's full mobilization. His bourgeois prejudice about women's place and his theories about racial breeding prevented a comprehensive draft law, like England's, which would have effectively tapped the reserve pool of female labor. Since his tender regard for German womanhood was socially discriminatory, affecting only those who had no need to work, it aroused resentment among overburdened working-class women and their husbands at the front. The forced labor of conquered peoples alleviated the labor shortage, however, and papered over the class conflicts that kept erupting through the veneer of class consensus *völkisch* ideology. Thus, the early success of German warfare supported Hitler's personal (irrational) ideology, although, Winkler insists, it was not "opportunistic" planned to do so.

Winkler's interpretation is as naive as her research is thorough. Her exhaustive work with party and government documents shows clearly what she denies in her conclusion—that Nazi ideology, having promised to resolve the contradictions of a rapidly industrializing nation, simply mirrored and manipulated them. I would argue that one of them was the blurring of sex/gender roles and an accompanying related need to define them more sharply. This contradiction was due, in large part, to the rationalization of industry begun under the Weimar Republic. The process de-skilled labor and restructured the sex-segregated labor market, creating competition between the sexes for jobs and arousing anxiety about family life and sexual identity—hence the popular reaction emphasizing preindustrial (if nostalgically distorted) male/female spheres.

The Nazis promised a restoration of this lost world. As with so many promises, they delivered the opposite. Winkler shows that they even hastened modernization of the economy by the war they provoked, though she argues that their stress on the traditional sector retarded the process. In fact, however, industry rationalized even more, the tertiary sector mushroomed, and women rushed even faster off the farms. Half-baked attempts to force the least privileged into agriculture failed. Nazi ideology conflicted with their real trajectory. But was it sincere? Winkler says that for Hitler, at least, it was.

To absolve Hitler of the opportunism of his party and fault him merely for honest bourgeois prejudice favoring housewifery not only raises him above politics (which comes dangerously close to

apologia, surely not intended by Winkler), but also overlooks the importance of his personal representation, as the "little man" incarnate, of the bourgeoisie, especially the petit bourgeoisie. His protection of the women of that class was neither more nor less opportunistic than was that class itself, which, as Winkler points out, profited from the war economy. Thus, Winkler wastefully addresses herself to a straw man at the end of an otherwise very strong piece of scholarship.

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MARTIN BROSZAT *et al.*, editors. *Bayern in der NS-Zeit: Soziale Lage und politisches Verhalten der Bevölkerung im Spiegel vertraulicher Berichte*. Munich: R. Oldenbourg Verlag. 1977. Pp. 712. DM 38.

This lengthy volume of documents is the first fruit of a larger collaborative project on "resistance and persecution in Bavaria" during the Third Reich. As such, it marks an emerging shift in scholarly approaches to what remains, after more than three decades, a complex and often emotion-laden subject. If earlier studies dealt principally with the motives, strategies, and political conceptions of active resistance figures, more recent work shows an inclination to chart the "limits of Hitler's power" (E. N. Peterson) in the unreflective, politically ambiguous behavior of ordinary Germans pursuing the ordinary business of daily life. Martin Broszat and his able coeditors offer here the strongest brief to date for the grassroots perspective. Their main theme is the "interaction of social structures and political control" (p. 11); their purpose is to illuminate specific structural conditions and determine how these alternately hampered, facilitated, or modified Nazi campaigns for social and ideological coordination. This they do through a series of topical chapters (on working-class behavior, responses to official anti-Semitism, wartime morale, and the like) and one in-depth study of a single rural district. Illustrative material comes from a truly impressive array of sources: local police reports, parish visitation records, and periodic *Stimmungsberichte* from minor bureaucrats and party officials. As one would expect, editing is impeccable throughout. Brief but helpful essays introduce the themes and evaluate the sources for each chapter.

The result, at its best, is a kind of total history as told by petty functionaries—a mosaic of local experiences colored by the blue and white of Bavarian tradition. Most of the details are new, though few, in themselves, are particularly surprising.

Generally speaking, the evidence reinforces common sense and prior research, although it often adds new accents or widens existing avenues of inquiry, as in the comparison of Catholic and Protestant parish life or the important material on agricultural concerns (pp. 327–425). While the book has no single overarching thesis, its recurrent focus is the small town and the rural hinterland. Here, in the provincial matrix of inherited piety, *Stammtisch* cynicism, and gruff peasant humanity, where forcible removal of crucifixes from schools provoked vigorous protest and the forcible removal of Jewish fellow citizens did not, the subtle interplay of social structures and political dynamics achieves a clarity seldom found elsewhere.

Despite its scope, the book is necessarily suggestive rather than exhaustive. The vagaries of archival preservation cause an inevitable unevenness of coverage. Urban conditions receive only passing attention, while the book's chosen format precludes full treatment of all comparative implications. Future publications will presumably close the more obvious of these gaps and also provide some interpretive synthesis embracing the whole project. For now, Broszat and his colleagues have managed the virtuoso feat of celebrating mundane affairs without trivializing the broader issues these reflect. All in all, it is a performance worthy of note.

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FRANZ GSCHWIND. *Bevölkerungsentwicklung und Wirtschaftsstruktur der Landschaft Basel im 18. Jahrhundert: Ein historisch-demographischer Beitrag zur Sozial- und Wirtschaftsgeschichte mit besonderer Berücksichtigung der langfristigen Bevölkerungsentwicklung von Stadt (seit 1100) und Landschaft (seit 1500) Basel*. Summary in English. (Quellen und Forschungen zur Geschichte und Landeskunde des Kantons Basel-Stadt, number 15.) Liestal: Kantonale Drucksachen- und Materialzentrale. 1977. Pp. xix, 687.

The author of this excellent dissertation in demographic and economic history had originally intended to write a history of famine in the rural subject territories ruled by the city of Basel. For various reasons this did not prove feasible; but in the course of his archival research Franz Gschwind happened to discover the original records of five censuses taken between 1698 and 1743, whose existence had been forgotten completely. Together with seven later censuses taken between 1770 and 1850 these records form the basis for a detailed analysis of the interrelationship between population growth and early industrialization in this rural area during the eighteenth century. In order

to gain a long-term perspective Gschwind also traces the population development of the city of Basel from A.D. 1100 and that of its rural territories from 1500 to the present day.

As far as the city is concerned three major phases can be distinguished. First, from the time the first town wall was built around 1100 to the Black Death of 1348–49 the number of inhabitants increased steadily from 2,000 to 12,000. Then there followed a long period of stagnation, characterized by sharp fluctuations, which lasted for more than three hundred years. Between the Black Death and the last epidemic of 1667–68, plague ravaged the city no less than twenty-three times. Altogether these epidemics took a toll of about 50,000 lives. The losses were made up largely by in-migration: about 34,000 newcomers were naturalized during this period. As a consequence Basel's population fluctuated between 5,000 and 12,000. It then increased slowly but did not exceed 15,000 throughout the eighteenth century due to severe restrictions on naturalization imposed after the end of the plague epidemics. The third phase began after the end of the Napoleonic era. Stimulated by urban industrialization and in-migration, the city's population increased from 16,000 in 1815 to 216,000 in 1964. Since then the trend toward suburbanization has ushered in yet another phase of population decline; in 1976 it stood at 191,000 and a recent forecast projects 178,000 inhabitants by 1990.

By contrast, the rural areas subject to the city, consisting of one hundred and fifty square miles, experienced a steady population increase, interrupted by relatively short phases of stagnation. The first estimate for 1497 shows a total population of 5,000. Growth was rapid during the sixteenth century and reached a total of about 14,000 in 1609. By the end of the seventeenth century a rural population of 20,000 had reached the agricultural sustenance limits. It stagnated until 1740 when the introduction of a cottage industry—the weaving of silk ribbons—provided a new basis for further population growth. For more than a century thereafter population growth and the expansion of this cottage industry developed in close correlation. The population reached 44,000 in 1850 when the expansion of this rural industry came to a halt. By then, the transition from an agricultural to an industrial economy was in full swing. The spread of factory industry, commerce, and service trades permitted a rapid population increase to 60,000 in 1900 and 96,000 in 1950. Thereafter rapid suburbanization sharply accelerated the growth rate: where formerly the surplus population of the countryside had migrated to the city, the migratory flow was now reversed, carrying the population to 166,000 in 1970 with no end in sight.

I have emphasized here overall growth and migration but the author has also competently analyzed population density, household size, fertility and mortality, as well as the age and sex structure, occupational structure, and religious composition of Basel's rural population during the eighteenth and early nineteenth centuries. Interested American readers may be glad to know that the book contains a ten-page summary in English.

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FRANK MCARDLE. *Altopascio: A Study in Tuscan Rural Society, 1587-1784*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1978. Pp. ix, 226. \$22.50.

Altopascio was a small rural community on the frontier between the Medici Grand Duchy of Tuscany and Lucca. The historical significance of Altopascio in the period 1587-1784, according to Frank McArdle, resides in the fact that the vast quantity of surviving documentation—records of the dukes' administrators and communal and parish records—allows the historian to chart the economic performance of the estate and provides a means for the larger task of "reconstructing a rural society in the past" (p. 10).

In four valuable chapters on demography and the economy, McArdle demonstrates how Altopascio shared in the "general crisis of the seventeenth century," experiencing a precipitous demographic decline after 1647. Accompanying this decline was a severe economic dislocation (at its worst from 1695 to 1725), as revealed by vicissitudes in production and prices on the estate. McArdle demonstrates that this crisis led to crippling levels of indebtedness among the peasants because the economic system worked to the benefit of the exploitative ducal landlord. Capital investment and efficient reforms were also thwarted. In sum, "the crisis at Altopascio only aggravated traditional inequalities between landlord and tenant," and, furthermore, this "tragedy was not rooted in the soil but in the terms of men's relations with other men" (p. 217).

The rest of the book, the social reconstruction of Altopascio, does not succeed nearly as well; here the limitations of McArdle's methods and sources become apparent. A study based solely on records of a single estate cannot hope to provide a perspective from which to confront the problems of interpretation presented by the sources. Any attempt to reconstruct a society in the past must involve interpretation of the polysemous cultural constructs found in the sources. Otherwise the door is open to misinterpretation (see McArdle's

treatment of emancipation, pp. 135-36) and anachronism (for example, the discussion of bearing arms, pp. 199-200).

McArdle discusses the meaning of cultural constructs without providing citations to sources for his remarks. He treats the archival sources not as the locus for historical interpretation but as a body of statistical data. In his hands cultural constructs become so many norms to be rendered statistically. The explanation of such statistics, however, must involve more than the delineation of the class and economic factors that McArdle gives us (valuable as such information is). It must involve reconstruction of the complex cultural system of which these data represent only a finite number of functional moments and from which they derive their meaning. This methodological shortcoming is most evident in the final chapter on social grievances. To conclude that there were no violent uprisings in Tuscany (as opposed to France) because of the *mezzadria* system is to say little. Of course the peasants were dependent on their landlords, but one needs to know how this dependence was symbolically expressed, accepted, and modified in order to understand the cultural logic behind the nonviolence of the exploited Tuscan peasants.

Within the limits of its sources and methods, this book is a welcome contribution. Its strength lies in the presentation of demographic and economic "events." The "reconstruction" of Tuscan rural society, however, must await the exploitation of a greater variety of sources and their interpretation, which will permit the reconstruction not only of norms and events but of the cultural logic connecting them.

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JEAN NICOLAS. *La Savoie au 18^e siècle: Noblesse et bourgeoisie*. Volume 1, *Situations au temps de Victor-Amédée II*; volume 2, *Inflexions au siècle des lumières*. Paris: Maloine; distributed by Librairie de la Nouvelle Faculté. 1978. Pp. xvi, 538; xvi, 540-1, 242.

In recent years historians have seen exhaustive studies of the eighteenth-century peasantry and Jean Nicolas has now written what must be seen as the definitive study of the nobility and bourgeoisie of Savoy. This study, presented by Nicolas for the *doctorat d'état* at the Sorbonne, discusses every aspect of the elite of eighteenth-century Savoy. Naturally there is abundant detail on the numbers of noble and bourgeois families and the amount and form of their wealth. Since much, but not all, of the wealth of these families was concentrated in land, Nicolas analyzes climate, quality of land, and price cycles as well as the more ordinary informa-

tion on sizes of land holdings and percentages of land owned by various socioeconomic groups.

One of the most interesting parts of the study is an examination of family structure including those forces, such as illicit love affairs, that tended to disrupt family life. We are also given a clear image of the everyday life of the elite—what they ate and wore and how they amused themselves. The role of the elite in government at all levels, involvement in *cercles* and *sociétés*, the increasing secularization of society, and the astonishing proportion of Savoy noblemen with military service in either Savoy or foreign nations (in 1701, for example, 53 per cent of all men over 14 had either served or were serving) are all discussed in detail.

The main theme of *La Savoie au 18^e siècle* is a familiar one: the decline of the nobility and the rise of a bourgeoisie, with small peasants being crushed between the two. Nicolas, however, makes no sweeping generalizations. Rather, he amasses so much evidence that each of his conclusions is undeniable. For example he discusses the penetration of Enlightenment thought in the second half of the century in tandem with demographic and economic forces, without, however, implying a cause and effect relationship. American historians may be astonished to note the extent to which Charles-Emmanuel III (1730-73) demolished the seigneurial system in Piedmont, despite the anguished cries of the nobility that they were being ruined. Indeed Nicolas shows that between 1702 and 1787 the number of noble households in Savoy fell 37 per cent, from 795 to 500, with most of the property of the extinct families being seized by the bourgeoisie. Antagonisms and hatreds between noble and bourgeois, not evident early in the century, were apparent to all by the 1780s.

Nicolas's research is truly exhaustive, and he discusses sources at some length in the text and notes. The prose is clear and well written; hundreds of charts, tables, and maps, and dozens of photographs, several of which are in color and all of which were obviously chosen with great care, add immeasurably to the book. In addition, wonder of wonders, there are forty-five pages of index. This study is obligatory reading for historians of the eighteenth century, and will ensure that Jean Nicolas will be mentioned, along with Lefebvre, Soboul, Le Roy Ladurie, Goubert, Saint-Jacob, and Agulhon, among the finest social historians of the Old Regime and the revolutionary period.

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GIOVANNI BUSINO. *Vilfredo Pareto e l'industria del ferro nel Valdarno*. (Studi e Ricerche di Storia Economica

Italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1977. Pp. 920.

The activities of Vilfredo Pareto in Italy, prior to his succession to Léon Walras's chair of political economy at Lausanne, are little known. A good biographical sketch of these Tuscan years appears in Tommaso Giacomone-Monaco's introduction to Pareto's *Lettere ai Peruzzi*, and bits of information are scattered elsewhere. The publication of a series of Pareto's letters of this "Italian period" is, therefore, of great importance for those interested in the biography of this engineer-turned-theorist. The correspondence, drawn from two books in which the marquis more or less regularly transcribed the letters he drafted, sheds considerable light upon the iron industry of the Valdarno, with which he was associated during most of these years (1873-93), as well as on the economic and social history of postunification Italy.

Although the letters included in the present volume constitute only a part of the business correspondence of Pareto, who served as director of the *Società per l'Industria del Ferro* and then its successor the *Società Anonima delle Ferriere Italiane*, they pinpoint the host of difficulties confronting industrial activity in the peninsula and especially its strict dependence upon finance capital. Pareto's letters disclose how the banks sustained various companies by means of short- and long-term credit and reveal how their frequent intervention in the entrepreneurial process led to a certain confusion in the roles of the participants in industry.

The volume is divided into two sections: an account of the troubled Tuscan iron works followed by an appendix containing Pareto's letters. The study of the relationship between the *Banca Generale* and the iron works complements the findings of Antonio Confalonieri's first volume of *Banca e Industria in Italia*. Busino also provides valuable information on the problems inherent in the use of native lignite in the industry, the lack of capital for technological modernization that might have made the company competitive, the almost total neglect of market analysis, and the failure to learn from errors of the past. Thus it was not only the tariff policy of the state, which the free-trader Pareto decried, that contributed to the failure of this enterprise.

The letters do more than contribute to our knowledge of early industrialization in Italy for they reveal the thought and personality of the author of the *Trattato di sociologia generale*. Pareto emerges as self-confident to the point of being contentious, as a man who had ready explanations for his problems but was almost always intolerant of the shortcomings of others and quick to place responsibility for failure elsewhere. The manage-

ment, the banks, the railroads, the workers, and the government were, in turn, all subjected to his caustic criticism. Secure in his own engineering skills, he questioned the competence of others and doubted their good will. He constantly found fault with his superiors, especially Luigi Langer, the director general of the company, whom he eventually replaced. He lamented the slowness of the Italian administration, convinced that only the Turkish one was worse. Clearly the roots of Pareto's pessimism and alienation are to be sought in this twenty-year period when he was an engineer in Italy.

Giovanni Busino, who is editing Pareto's complete work, has incorporated all the correspondence of the letter books, including some letters published elsewhere in order to preserve continuity. His narrative, based in part on the correspondence, also draws on a wide range of archival and printed sources. He renders the letters intelligible and places the subjects Pareto discusses within the proper context. Busino's notes are authoritative as well as copious and help make this volume a very important source for the study of both the marquis and the economic history of post-*Risorgimento* Italy.

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Jamaica, New York

DAVID W. ELLWOOD. *L'alleato nemico: La politica dell'occupazione anglo-americana in Italia, 1943/1946*. (Istituto Nazionale per la Storia del Movimento di Liberazione in Italia. Saggi e Biografie, number 384.) Milan: Feltrinelli. 1977. Pp. 454. L. 12,000.

Italy, being the first European area to be liberated by the Western Allies in World War II, provided a laboratory for the testing of Anglo-American war aims. The British emphasized Italy's role as an Axis partner to justify subjecting the country to a kind of imperial domination. The United States, however, looking more to the future than the past, proposed to rehabilitate Italy by New Deal methods and to integrate her spiritually into a new world order based on Western concepts of freedom and justice. In due course the American vision prevailed and became the philosophy of official Western policy and a propaganda instrument in the burgeoning Cold War. But alas for intentions both nefarious and noble. The exigencies of military campaigning as well as the omnipresent need to feed, clothe, and house the liberated population constrained the Allied military government to turn pragmatically to whatever would serve in an emergency, for the most part to the surviving apparatus of the pre-Fascist order. By 1946 Italy had thus settled, albeit not at its own behest, for tradition

and stability in lieu of change and reform. It all amounted to yet another in the long line of Italian *rivoluzioni mancate*.

In outline, this interplay of Allied strategy and Italian domestic politics constitutes a familiar enough tale. David W. Ellwood tells it, though, with more authority and thoroughness than previously. Norman Kogan's *Italy and the Allies* (1956)—oddly missing from Ellwood's otherwise comprehensive bibliography—was an early and still valuable survey, while C. R. S. Harris's *Allied Military Administration of Italy 1943-45* (1957)—a volume in the official British *History of the Second World War* series—rested on limited documentation drawn from Allied Forces Headquarters in the Mediterranean. In contrast, Ellwood has ransacked the archives of three countries, much of his material having become accessible only in the last decade or so. Most of his information comes from the great national depositories—the Public Record Office, the National Archives, and the Archivio Centrale dello Stato—but much also has been garnered from the libraries of the various Italian institutes for the study of the liberation.

L'alleato nemico began life as a doctoral dissertation at Reading University, England, and betrays something of its origins in a tendency to overorganization and repetition. For instance, the work is arranged topically in three sections—"Italy in International Relations 1943-45," "Military Requirements and Political Objectives: The Nature of the Allied Control," and "The Political Economy of Stabilization"—which may strike some as too schematic to capture the gradual unfolding of the entire scenario as it must have appeared to participants at the time. But this is no more than a minor quibble. What was plainly an excellent doctoral thesis has been turned into a very good book that will remain the standard work on the subject for some time.

ALAN CASSELS
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RONALD E. COONS. *Steamships, Statesmen, and Bureaucrats: Austrian Policy towards the Steam Navigation Company of the Austrian Lloyd, 1836-1848*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 74.) Wiesbaden: Franz Steiner Verlag. 1975. Pp. xiii, 209.

This elegantly documented little monograph does exactly what its awkward subtitle promises. Chapter one recounts the outrage and suspicion of Count Joseph Sedlnitzky, Emperor Francis's police president, when he discovered in 1833 that various merchants and insurance institutions at Trieste had, with the cooperation of the provincial

governor, established an information gathering agency, the *Lloyd Austriaco*. "Austria's policy" toward this forbear of the Austrian Lloyd Steamship Navigation Company was to treat it as a potential breeding ground of revolution and to plant a state supervisor on its board of directors. Next, chapters two, three, and four describe the founding of the steamship company itself in 1836 as a private commercial undertaking and the successive stages by which Vienna overcame its free trade principles: first the government gave the company a lucrative postal contract, then in 1839 underwrote its towering debts, and then in the mid-1840s underwrote an annual dividend of 4 percent to the stockholders and enabled the company to carry the Austrian flag to Trebizond and Alexandria despite increasing competition from the French and British. A final chapter recounts Metternich's unsuccessful diplomatic efforts in the later 1830s and in the 1840s to gain entry for the Lloyd's ships to the Adriatic ports of the Kingdom of Two Sicilies and to win the company a contract to carry the British East Indian mail between Alexandria and Trieste. The conclusion of the book is that the *Vormärz* Austrian government was more sensitive to economic interest than has often been supposed.

Ronald E. Coons writes well, has a sense of humor, and has done a model research job. He has dodged from the Staatsarchiv to the Hofkammerarchiv and to the Wallnergasse. He has delved into archives in Trieste and Venice and has used the Public Records Office in London and the National Archives in Washington. One can learn here an enormous amount about how various lethargic branches of the *Vormärz* Austrian bureaucracy administered without governing. Yet the book is oddly lacking. In part this is because the archives of the Lloyd company itself no longer exist; a straightforward history of the company would have been difficult. But the trouble lies also with the author's unwillingness to look beyond his precise subject. In effect, he leaves huge lacunae. He generalizes with facility and even brilliance about numerous topics that he claims to have no time for, so the reader should have no doubts about his ability to probe further. But, for example, he affords one virtually no idea of why the company at first did badly in its operations in the Levant and then later did well. Though they were clearly available to him, he made no use of the Austrian consular reports from the Ottoman Empire (not to speak of the British and French consular reports), which might have shed light on how the company grew. He seems as indifferent to economics as were Metternich and Kolowrat.

Moreover, although this book ends with a discussion of whether free enterprises should accept state help, it leaves even the central commercial

operations of the company at Trieste singularly anonymous and vague. There is only a mention of Salomon von Rothschild in an early chapter. One gathers that the company directors were willing to pay dividends even when the company was deeply in debt. But at the end the reader is so little informed that he can still ask what was the role of free enterprise in this firm? Most astounding of all, the very name of Carl Ludwig von Bruck appears only half a dozen times, all in one chapter. The reader might have hoped to find out here how Bruck, the general director of the company, became eligible during the *Vormärz* for the exalted role he played at Vienna in 1848 and after. It was he, after all, who putatively obtained all those favors from Vienna for the Lloyd, and the book is *de facto* about his early career. But he just is not there. For such reasons these entertaining contributions to Austrian commercial history are disappointing.

WILLIAM O. MCCAGG, JR.
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ILONA DUCZYNSKA. *Workers in Arms: The Austrian Schutzbund and the Civil War of 1934*. With an introduction by E. J. HOBBSBAWM. New York: Monthly Review Press. 1978. Pp. 256. \$15.00.

As an avowed Marxist and a participant in the Austrian labor movement, Ilona Duczynska has written a sympathetic, yet insightful and interpretive, history of the *Schutzbund* during the interwar period. Although the climax of the book is the abortive uprising of 1934, most attention is devoted to the development of the paramilitary organization, its relation to the Social Democratic Party, and the changes it underwent prior to 1934 that reduced it from a group of dedicated revolutionaries to a misguided cohort hampered by indecision, inadequate preparation and tactics, and vacillating leadership. The author views the *Schutzbund* as the epitome of revolutionary consciousness, which would have succeeded in its proletarian revolution if it had not been weakened by its association with and ultimate subordination to the Social Democratic Party.

During the 1920s the Austrian Social Democratic Party was preoccupied with internal debates on what the role of the *Schutzbund* should be. The decisions resulting from the discussions, Duczynska maintains, established party dominance over the *Schutzbund*, removed it from the political arena, and caused it to adopt tactics that were militarily unwise and that lessened the workers' initiative and morale. The chief culprits in sealing the doom of the *Schutzbund* were Otto Bauer and Julius Deutsch, who viewed the militant organiza-

tion not as a force to fight a civil war, but as one whose existence would prevent an enemy from starting a conflict. The threat of the *Schutzbund*, which was well armed and larger than the national army, was never effective, however, because it existed only to prevent rightist encroachment, not to win a war. On several occasions, all of which are discussed at length, the *Schutzbund* could have been used to prevent a gain by rightist forces, but was prevented from doing so because Bauer shrunk back in fear of a civil war. By 1926 the *Schutzbund* had become "militarized," that is, nonpolitical, and subject to the direction of the Social Democratic officials. In addition, the party had adopted a list of extreme conditions that would have to precede an armed revolt. When the Linz *Schutzbund*, feeling it could bear no more, spontaneously rose up in 1934, the Social Democratic Party was unprepared; the call for a general strike went partially unheeded, many *Schutzbund* units were inoperative because they had been trained to rely on commands from above, and numerous leaders had allowed themselves to be arrested the previous day.

The deterioration of the *Schutzbund* could have been avoided if the author's hero, Theodor Körner, had been heeded. A former military officer, Körner stressed the political education of the *Schutzbund* and the adoption of guerilla tactics but was not allowed to carry out his plans of making the *Schutzbund* capable of winning a civil war and effectively exercising power in the aftermath. Coupled with the failure to hearken to Körner's wisdom was the inherent contradiction of Austro-Marxism: the espousal of revolutionary doctrine but the use of democratic, parliamentary methods. Allegedly, the Austrian workers were dedicated Marxists but were averse to the notion of the proletariat's seizing power. Also, organizational and propagandistic efforts were contrary to the workers' revolutionary goals, thus consigning the *Schutzbund* to failure.

Although the author fails to present the triumph of the Right in the context of contemporary Europe and her assumptions regarding the revolutionary ardor of the Austrian workers and the correctness of Körner's methods can be questioned, the book, which is based on party documents and interviews with participants, presents a detailed, valuable, and personal insight into the transformation of the *Schutzbund* and an internal view of the reasons for the failure of the workers' movement. It is only slightly marred by Marxist revolutionary jargon and the author's leftist orientation.

BLAIR R. HOLMES
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LUDWIG JEDLICKA and RUDOLF NECK, editors. *Das Juliabkommen von 1936: Vorgeschichte, Hintergründe und Folgen*. (Veröffentlichungen des Wissenschaftlichen Kommissions des Theodor-Körner-Stiftungsfonds und des Leopold-Kunschak-Preises, number 4.) Munich: R. Oldenbourg Verlag. 1977. Pp. 480. DM 48.

The fourteen papers collected in this useful book were presented in 1976 at the Vienna symposium on the July 1936 Agreement concluded between Austria and Nazi Germany. This fourth volume in the collection of essays from the annual symposia organized by the Scientific Commission for Research into Austria's History from 1927 to 1938 reconstructs a number of particularly complex and elusive problems of that period and is fresh evidence of the vitality of current concern with recent Austrian history. It has as editors the late Ludwig Jedlicka, whose studies of contemporary Austria are too well known to need comment, and Rudolf Neck, who has given us valuable works on the Austrian labor movement. The essays are written with insight, and the appended discussions add to the value of the work. The index is limited to persons.

The carefully documented articles fall roughly into three categories. The first group, forming the heart of the book, centers on foreign policy. Hanns Haas provides a searching investigation of French and Soviet policies toward Austria in 1936. Reinhold Wagnleitner and Walter Hummelberger concentrate on the British-Austrian relations and the role of the Little Entente, respectively. Jedlicka examines the position of Italy, whose involvement in the Abyssinian war adversely affected the international situation of Austria. The domestic problems of the Austrian authoritarian regime relating directly and indirectly to the July Agreement are treated in the second category of wide-ranging essays: Peter Broucek on the representative of the national opposition, Edmund Glaise-Horstenau; Wolfgang Neugebauer on the illegal labor movement; and Gerhard Jagschitz and Gustav Spann on the Austrian Nazis from 1934 to 1936. Of particular interest is Anton Staudinger's study on the concept of Austrian identity. The third group of articles is concerned with the economic aspects of the Austro-German relationship: Isabella Ackerl supplies evidence of the financial scandal connected with the Phönix Life Insurance Company; Norbert Schausberger traces the economic background of the July Agreement; Peter G. Fischer and Karl Haas survey the activities of the chambers of commerce and other economic associations, respectively; and Karl Stuhlpfarrer outlines

the problems of the economic penetration of Austria by Germany.

As collections of essays go, the volume is, overall, of quite respectable quality; there are more sprightly, suggestive essays than there are pedantic ones. Some of the contributions address themselves directly to the theme; others are special treatments, all useful, in the field of the authors' specializations, with adaptations to the subject matter. The articles are generally more impressive individually than as a group because no organized analysis of the July Agreement emerges. The prevailing opinion of the authors is that a serious blunder was committed by Federal Chancellor Kurt Schuschnigg. They believe that the agreement helped deepen the Nazi peril to Austrian independence. A few contributors, however, show some degree of charity toward Schuschnigg. Although critical of his conduct of foreign policy, they appear to understand that in the circumstances he had little room for maneuver within the framework of a virtually inevitable series of choices.

The volume, predominantly based on original research, provides a valuable addition to the historiography of modern Austria. Anyone interested in modern Austrian history will find something new and useful here by way of either fact or insight.

RADOMIR V. LUZA
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VÁCLAV ŽÁČEK *et al.* *Češi a Jihoslované v minulosti: Od nejstarších dob do roku 1918* [The Czechs and the Yugoslavs in the Past: From Ancient Times to 1918]. Prague: Academia. 1975. Pp. 751. Kčs 98.

Czech and South Slav interaction, Czech and South Slav migrations, Czechs and South Slavs compared—such are the subjects treated here. With little mention of the Slovaks, Bulgarians, or Macedonians, it is rather the interaction and influence of the Czechs on the Serbs, Croats, and Slovenes from the days of the great *Völkerwanderung* to 1918 that is the object of attention.

Two chapters by Lubomír Havlík, based on a wide range of primary and secondary sources, are devoted to the medieval experience from the sixth to the fourteenth centuries: migrations, Christianization (both Roman and Orthodox), the impact of the investiture controversy, the Habsburg-Přemyslid clash, and liturgical controversies. The degree of interaction between the Czechs and the South Slavs was often not particularly significant, but the judicious, careful handling of the sources and the clear maps are an asset to the study.

Closer and more frequent contacts developed in the fourteenth and fifteenth centuries (chapter three, by Jaromír Mikulka) as Bohemia and Croatia came under the same dynasties, but neither kingdom was significantly influenced by the other, despite Hussitism, the dynastic wars, and the Turkish peril. Quite possibly this chapter could have been strengthened by a better analysis of the failure of Hussitism in the South Slav areas, even at the expense of omitting mention of the movements of individual clerics and soldiers.

It was the Turks and their advance up the Balkans that proved decisive in Eastern Europe and one of the *raisons d'être* of the Habsburg monarchy. The defense against the Turk and his ultimate expulsion from Europe required centuries of brutal, grim warfare, with the South Slavs in the front line. But the Czechs were affected too, and this is described in chapter four (by Zdeněk Šimeček) and in the first portion of chapter five (by Milan Šmerda) and again in chapter eight (by Václav Žáček and Růžena Havráňková) in the context of the great eastern crisis of the 1870s.

Individual Czechs fought as soldiers and commanders, took part in embassies to Constantinople, married South Slavs, and paid heavy taxes for the wars of the sixteenth and seventeenth centuries. Czech-South Slav interaction was furthered by the migration of large numbers of Croats northward, especially in the 1630s. Especially interesting to the student of Balkan history is the discussion of Czech documentary sources on Balkan conditions. The seventeenth century brought disaster to the Czechs and some mutual antipathy: Czech Protestantism offended South Slavs, and Croatian atrocities in Bohemia in the Thirty Years War angered the Czechs.

The second half of the eighteenth century brought about more interaction between Czechs and South Slavs; but, more importantly, it witnessed the beginnings of the Czech national revival. Discussed by Milan Šmerda in chapter five and Václav Žáček in chapter six, the revival's origins and its impact on the Slovenes, Croats, and Serbs are well described. Economically and culturally more advanced, the Czechs would remain the tutors of the South Slavs for the next century. They took a lively interest in the struggle of Vuk Karadžić against the conservative Orthodox clergy in the interest of linguistic and orthographic reform, which, it is asserted, "strengthened the hand of the young bourgeoisie" (p. 227). Žáček's otherwise solid description of the cultural revival could have been improved by better attention to the role of some of the nobility and indeed the Austrian government, especially Count Kolowrat.

The revolutionary upheavals of 1848 take on the

aspect of a Slavic struggle against Magyars and Germans in chapter seven (by Žáček). This chapter is quite good on the Slav congress in Prague and the embarrassments caused by Jelačić. But there is not much on military events, nor on the Reichstag deliberations. Relations and political problems between Czechs and South Slavs continued to be complicated in the years between 1850 and 1890. This is well understood by the authors of chapter eight, Žáček, Havráňková, and Šestár. Czechs in the Vienna parliament found themselves at odds with Croats from Dalmatia, and relations between Rieger and Strossmayer cooled considerably by the 1890s. Czechs, however, were generally pro-Serb, especially during the crisis of the 1870s. About the only exception was Czech sympathy for Bulgaria in the 1880s. Czech support for the Croats sometimes produced money for electoral campaigns; cultural contacts steadily increased.

The Young Czechs, discussed by Vladislav Štastný in chapter nine, do not come off well. "So called progressives" and "spokesmen of the Czech bourgeoisie" (p. 589), they gave only lip service, he asserts, to the cause of the South Slavs. This chapter includes a discussion of Neo-Slavism by Karel Herman and of the Badeni language ordinance and its repercussions but is less informative on the implications of the ordinance and seems oblivious to the other serious crisis in the monarchy, the rift between Austria and Hungary. Although Masaryk is characterized as possessed of "high personal political ambitions" (p. 562), "the bearer of bourgeois reforming ideas" (p. 560), and favorable to Austrian peaceful expansion in the Balkans in the interests of Czech capitalism (p. 561), his influence on the South Slavs is developed (in chapter nine and in chapter ten by Miroslav Tejchman) at considerable length.

Czech-Yugoslav relations in World War I were generally cordial. Via various routes Czechs found their way into the Serbian army, and the Serbian government provided diplomatic support for Masaryk. But tensions arose, primarily from the dubious Treaty of London. Not only did this result in tension between Italians and Yugoslavs, but as the Czechs attempted to mediate they annoyed the Yugoslavs and quarreled among themselves. There is also discussion of the Congress of Nationalities and the rather dramatic mutinies in the Austrian fleet in 1918.

Most Western scholars will find it difficult to agree with the enthusiastic remarks about the Soviet Union in the introduction, but this is still a valuable work, of interest to students of Czech, South Slav, and Austrian history. Its value is enhanced by about thirty pages of bibliography, which could have been improved by more attention to Western sources, and over a hundred illus-

trations. The most important improvement would have been a concluding essay, summarizing and analyzing the subject. A translation into English would be welcome.

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NICOS P. MOUZELIS. *Modern Greece: Facets of Underdevelopment*. New York: Holmes and Meier. 1978. Pp. xiii, 222. \$25.75.

Southeastern Europe, whose peoples have shared a common historical past but today diverge significantly in their governmental and economic systems, is a prime area for research in the comparative development of societies. Yet Western scholarly interest in this region has slackened noticeably in recent years, partly in response to the vicissitudes of international power politics. In marked contrast to this trend, significant scholarly work has appeared in Greece since the restoration of parliamentary government in that country in 1974. The purpose of a good deal of this Greek work is to analyze the evolution and present functioning of Greek society. Many of the authors of these critical studies have been educated abroad, and their work reflects an impatience with and criticism of traditional views of their country's historical development. Nicos P. Mouzelis's book is one of the most vigorously argued and thought-provoking of these works.

Modern Greece is a study in historical sociology. It is enhanced by the author's critical historical vision and at the same time weakened by his reliance on the jargon of his discipline when clear English would have sufficed. Half of the chapters have already been published elsewhere so that the book is, in effect, a series of related but distinct essays. Bringing these essays together under one cover and integrating them with those written as part of the book results in some overlapping and repetition.

The author has combined sociological theory centered on a class analysis of developing societies with a historical survey of modern Greece's socioeconomic development in order to demonstrate, as he sees it, the inadequacy of traditional and neo-Marxist theories of modernization as well as the imbalanced and often noncreative nature of Greek society. He commendably recognizes the need to avoid a mechanistic framework that leaves no room for the impact of personalities but still seeks to go beyond surface events in his examination of modern Greek development. Yet his investigation of Greece's history, based as it is on a class analysis, leads to categorical assertions—such as

those concerning Venizelos's role in the bourgeois transformation of the country (p. 21) and the significance of foreign capital in the post-World War I era (p. 23)—that will be open to question.

In his discussion of topics such as the diaspora Greek bourgeoisie of the nineteenth century, the development of agriculture, the rise of a capitalist economy in Greece, and the role of the military, Mouzelis is sensitive to the question of the availability of data. Although the book contains hundreds of footnotes, the author has had to rely heavily on secondary accounts. His recourse has been to try to find a middle ground between grand but unsubstantiated theory and a timorous reliance on facts. A good deal more primary research by scholars is still necessary, however, to test the hypotheses in studies such as this one.

Modern Greece has been shaped (Mouzelis would say warped) by a series of political and economic forces that may be arranged in three concentric and expanding circles. The innermost represents the world of the Greeks indigenous to the state when it was created. They developed a society that was a product of their particular interests compounded with the influence of the two outer worlds. Mediating between the inner and outer ring were the "outside" Greeks, whether in Ioannina, Vienna, Liverpool, or Chicago. These diaspora communities have acted as a force shaping as well as conducting the economic, political, and cultural currents of the outermost circle: Western Europe and, more recently, the United States. The overall merit of Mouzelis's work is that it delineates and relates these three convergent worlds into an integrated system and thus provides a proper perspective for the study of such a society.

By concluding his work with a plea for a path of development for Greece other than the capitalist or state socialist models provided by Europe, the author points up the need for more intensive study of societies like this one.

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LEON ȘIMANSCHI, editor. *Petru Rareș*. Summary in French. Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 336. 24.50 L.

Petru Rareș is an important contribution to Rumanian history and historiography. The book was published in commemoration of the four hundred and fiftieth anniversary of the accession to the throne of Moldavia of yet another Rumanian *voevod* who could be regarded as a forerunner of Nicolae Ceaușescu, the present ruler of Rumania.

In the preface to this collection of sixteen studies

on various aspects of Rareș's two turbulent reigns, the eminent Rumanian historian Ștefan Ștefănescu offers the rationale for the volume by stating that the political philosophy of Petru Rareș provided the impulse for the actions of great leaders, of history makers, committed to strengthening and elevating the Rumanian fatherland. Rareș is thus depicted as a valiant fighter for the maintenance of the historic legacy of his father, Stephen the Great, who had defeated both Pole and Turk in constant struggles for the preservation of Moldavia's independence. Because of his constant participation in Transylvanian affairs in the years following the battle of Mohacs and the ensuing struggle for power among Turks, Habsburgs, Poles, and John Zapolya for control of the Hungarian lands, Rareș is also regarded as a forerunner of Michael the Brave. Finally, Rareș is presented as the representative of the interests of the Rumanian masses oppressed by treacherous boyars and abused by foreign armies.

The authors of individual chapters solve the problem of reconciling the historic evidence with the political necessities of the moment in an original manner. Eleven of the sixteen contributions are concerned exclusively with thorough analyses of data pertaining to domestic and foreign affairs. The remaining five, bearing such titles as "Precursor of Michael the Brave," "Defender of the Inheritance of Stephen the Great," "The Family of Peter Rareș," "Ideological Confrontations," and "The Ruler's Personality," although impeccable from a scholarly standpoint, do address themselves expressly to present-day issues and, more by implication than by direct statement, allow for the substitution of Nicolae Ceaușescu for Petru Rareș. It is fair to say that similarities do exist and that the historic evidence has not been mutilated in the process.

Historians concerned primarily with new data and interpretations, however, will be disappointed. The only truly original contributions in this work are those that clarify hitherto obscure aspects of Habsburg diplomacy in Transylvania and Rareș's skillful exploitation of the complexities of the Hungarian conflicts through his ever-shifting dealings with contending parties. Simplified explanations of the aims and policies of the Porte and of Poland toward Moldavia, deliberate exaggeration of Rareș's own concern with the preservation of Rumanian interests in Transylvania, and attribution of unrealistic motivations to other actions, foreign and domestic, by the Moldavian ruler are also evident in this work *à thèse*.

Most of the authors of the volume, including the editor-in-chief Leon Șimanschi, are relatively unknown in Rumanian historiography. Their competence augurs well for the future of studies con-

cerned with the history of Moldavia, until recently much neglected.

STEPHEN FISCHER-GALAŢI
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BÉLA KÖPECZI. "*Magyarország a kereszténység ellensége*": *A Thököly-felkelés az európai közvéleményben* ["Hungary the Enemy of Christianity": The Thököly Rebellion in European Public Opinion]. Budapest: Akadémiai Kiadó. 1976. Pp. 371. 96 Ft.

The 1664 Treaty of Vasvár, concluded between the Habsburg emperor, Leopold I, and the Ottoman sultan, Mehmed IV, secured peace between the two empires on the Hungarian frontiers. The Viennese court used this opportunity for an attempt at forced integration of the Hungarian province to the absolutist Habsburg Empire. The majority of the Hungarian landed estates resented this infringement on their traditional privileges, the higher taxes, and the particularly heavy-handed efforts of proselytization of the Protestant masses to the Roman Catholic Church. The anti-Habsburg elements rallied around the Palatine of Hungary, Ferenc Wesselényi, who attempted to solicit Ottoman aid for his national movement.

After this conspiracy's failure, Leopold I accelerated the integration process. In 1673 he suppressed the Hungarian constitution, abolished the office of elected palatine, and appointed the German, Johann Ampringen, as governor of Hungary. Public reaction was bitter, and for many open resistance was the only possible answer. In 1678 Imre Thököly, a young Hungarian aristocrat who had lived in Transylvanian exile since 1670, took over the leadership of the insurgents; after acquiring French and Ottoman financial assistance, he succeeded in establishing himself in Upper Hungary. At this point Grand Vizier Kara Mustafa decided to take a more active role in the insurrection in order to further his own ambitions. When the truce between the Habsburgs and the Turks expired in 1683, his army advanced to Vienna and besieged the Habsburg capital. Meanwhile Thököly's *kuruc* bands attacked Pressburg. It took the united armies of Charles of Lorraine and John Sobieski to defeat the Turks and their *kuruc* auxiliaries. During the following fifteen years the Ottomans were driven out of Hungary and Thököly's fortunes changed accordingly. He had to flee to Ottoman soil, where he died in Izmit just a day before the Hungarian diet at Szécsény elected Ferenc II Rákóczi, the new leader of the anti-Habsburg movement, Prince of Hungary (September 12, 1705).

Throughout Europe people followed Thököly's exploits with keen interest and read avidly the

hundreds of publications concerning the Hungarian insurrection. In the present work, Bela Köpeczi, an authority on the *kuruc* movement, examines European public reaction to Thököly. He methodically culled through the available contemporary literature published in Western Europe thodically culled through the available contemporary literature published in Western Europe between 1664 and 1699, checking newspapers, periodicals, pamphlets, books on geography and history, and even fiction, in order to determine the writers' attitudes to the Hungarian dissenters. The bulk of Köpeczi's work consists of summary descriptions of these publications and their content analysis. This sort of research must have been time consuming; in chapter four alone, Köpeczi evaluates some 163 publications written in French, German, Latin, English, Dutch, and Italian—a task possible only through persistence and proficiency in several languages.

Analyzing his source material, the author carefully demonstrates that by the seventeenth century the printed word weighed heavily in forming public opinion and also that the European governments, aware of the increasing importance of the press, attempted to control its content. This use of the press for societal manipulation was most obvious in France. In a variety of publications French writers resolutely defended Thököly, arguing that the Hungarians were forced to rebellion because of the oppressive character of Habsburg rule. In the French view Thököly had no choice but to turn to the Turks for assistance in order to continue his anti-Habsburg insurrection. In this positive evaluation the French were alone in Europe. Writers from the rest of Western Europe were all deeply hostile toward Thököly, especially after 1688, when the Grand Alliance of the Habsburgs, Bavaria, Saxony, Sweden, and Holland began a concentrated bombardment of anti-French propaganda. These writings bitterly blamed Thököly for his rebellion against his legal sovereign, Leopold I, just when the emperor was liberating Christian Hungary from Muslim servitude. In their judgment, cooperation with the common Muslim arch-enemy amounted to an open betrayal of the Christian commonwealth.

By buttressing his argument with numerous quotations, Köpeczi succeeds in demonstrating that the Thököly question in the European press, though under the guise of Christian solidarity, was in reality politically motivated. The underlying cause for these attacks was the fact that the writers considered Thököly but a puppet of Louis XIV.

This unfavorable image of the Hungarians in the anti-Thököly literature generated a lasting impression throughout Europe and influenced public opinion long after the demise of the *kuruc* movement. According to this picture, the fall of Hun-

gary was merely a result of innate Hungarian personality traits: greediness, inner discord, inclination for rebellion, and, above all, an inimical dislike of Germans. The author considers the Thököly period crucial in establishing this Hungarian image in the European mind. Probably this conclusion is the most significant result of Köpeczi's laborious research.

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FRIEDRICH GOTTAS. *Ungarn im Zeitalter des Hochliberalismus: Studien zur Tisza-Ära (1875-1890)*. (Studien zur Geschichte der Österreichischen-Ungarischen Monarchie, number 16.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1976. Pp. 257. DM 60.

The subject of this book is new, at least in a Western language, because, as the author himself explains, non-Magyar historians of dualistic Hungary have concentrated so far on the Ausgleich of 1867, on the final two decades of the monarchy, or on such specific problems of the period as the nationality struggle, the Hungarian *Kulturkampf*, and the Jewish question. Yet the age between 1867 and 1900 is just as fascinating, especially those fifteen years when Prime Minister Kálmán Tisza was the undisputed leader of the country.

This immensely wealthy Protestant landowner, of pure Magyar stock, who was as much feared as he was respected, laid the foundations of modern Hungary. He did so, Friedrich Gottas explains, by increasing Hungary's international reputation, by improving his country's position vis-à-vis the Austrian partner, by stabilizing and centralizing the administration, by fostering rapid economic development, and by bringing non-Catholics close to complete equality in this Catholic kingdom. The author sees Tisza as a convinced liberal who was, however, also a Magyar chauvinist and a man of authoritarian methods. Tisza helped the Jews, and he energetically countered the anti-Semitic outbursts that followed upon the Tiszaeszlár ritual murder case in the 1880s. He based his economic policy on the principle of free trade; but, under him, as under almost all the prime ministers of Habsburg Hungary, parliamentarianism was marked by one-party rule and the hegemony of the "Government Party," and minority nationalist as well as socialist agitation was ruthlessly suppressed. Actually, Tisza's chief difficulties lay not in domestic affairs—such grave domestic problems as land reform were conveniently ignored, to haunt the nation later—but in Hungary's relations with the Austrian half of the monarchy. In this matter

the prime minister proved himself both energetic and cautious: without ever endangering the 1867 Ausgleich, he wrung the maximum number of concessions from the emperor-king and the Austrian government. Although Hungary contributed only 30 percent of the joint expenses of the monarchy, the Magyar leader established virtual political superiority over the Austrian partner. The name of the joint army was changed from the Imperial-Royal Army to Imperial and Royal Army (the little *und* symbolizing Hungary's utter determination to continuously assert her independence); the Austrian National Bank gave way to the Austro-Hungarian Bank in which Hungarians had an equal say with the far wealthier Austrians; and the customs and trade agreement was revised in Hungary's favor.

Behind it all, Gottas points out, loomed Tisza's resolve to preserve the domestic hegemony of the Magyar nobility and to refuse any concessions to the Slavic and Rumanian minorities. On the contrary, the assimilation of the non-Magyars was pursued with vigor, although not yet with the aggressiveness of later regimes. The author is then perfectly right in concluding that Tisza was a typical representative of the nobility, the social stratum to which he belonged. The story is told soberly, with more than adequate documentation (the bibliographical footnotes and appendices are particularly valuable), but with no sense of humor, no irony, and no palpable feel for the human element in politics. It is a pity, indeed, that a scholarly work devoted to the extraordinary accomplishments of a powerful statesman should be so short of information on the personality of that statesman or on how decisions were really taken. The key to the secret room is missing.

ISTVÁN DEÁK
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BÉLA K. KIRÁLY and PAUL JÓNÁS, editors. *The Hungarian Revolution of 1956 in Retrospect*. With an introduction by G. H. N. SETON-WATSON. (East European Monographs, number 40; Brooklyn College Studies on Society in Change, number 6.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. x, 157. \$11.00.

The twentieth anniversary of the Hungarian Revolution, which erupted on October 23, 1956 and was crushed two weeks later, occasioned this collection of fine essays. The authors—prominent scholars, former revolutionaries, or both—reexamine its causes and consequences.

G. H. N. Seton-Watson's introduction is a veritable review of the book's contents. Noting the

lessons learned by the Soviet Union, he indicates that the West's indifference contributed to the Soviets' aggressive self-confidence in Eastern Europe. He also recognizes that Hungary was given the opportunity to experiment with a more humane form of socialist system. The latter observation is supported by Andrey A. Amalrik's brief analysis, in which he attributes Hungary's relatively high standard of living to the temerity of the 1956 revolutionaries.

Selected essays by the two major Marxist participants in the revolution, the Communist Imre Nagy and the socialist Anna Kéthly, follow. Nagy, arrested by the Russians on November 23, 1956, was executed in 1958, ostensibly for revolutionary activities. Tibor Méray, however, using thus far untapped Chinese sources, claims that he was, in fact, "the victim of Khrushchev's difficulties with the Chinese and Yugoslavs" (p. 78). Nagy's treatise was written during the intraparty struggles in 1955. He called on his comrades either to abandon Stalinism in favor of reform or to face the possibility of a revolution. As indicated by the reflections of Tamás Aczél, George G. Heltai, and Paul Jónás, young intellectuals rallied behind Nagy to stave off catastrophe. Nagy wished to give new meaning to democratic centralism and Leninism. His defense of these concepts, however, makes debatable the claim that he was either a "proto-Eurocommunist" (Seton-Watson, p. 6), or one of the founders of Eurocommunism (Stephen Borsoody, p. 129). Anna Kéthly, who died in exile shortly before the twentieth anniversary, dismissed the argument that Kádár's Hungary achieved the aims of the revolution. Its basic goal, self-determination, was not accomplished and, she claimed, Hungary continues to be exploited by the Soviet Union.

These essays indicate, however, that even during the uprising, self-determination was not carried through to its limits. The threat of Soviet intervention tempered the revolutionary designs of most. Despite this moderation, the Soviet army invaded, and the defense forces, under the command of Major General Béla K. Király, engaged the aggressors. Király, now Professor Király, in his own contribution to this volume, sees this episode as the first war between socialist states.

Another group of essays treats the reactions of Hungary's socialist neighbors and Poland. The revolution inspired the "domestication" of socialism. The term is used by Adam Bromke in his analysis of Polish developments after 1956, matched by those in Rumania. Stephen Fischer-Galați shows that Ceaușescu's brand of national communism was a response to the events in 1956. Paul E. Zinner observes that the leaders of the "Prague Spring," recalling Czechoslovakia's

unique democratic tradition, justified their attempt to domesticate socialism and to give it a "human face."

The last three articles deal with the revolution's impact on the West and demonstrate that Russian intervention contributed to the low esteem in which Western political groups hold the Soviet system. For this reason, even Western Communists seek an alternative in Eurocommunism.

The editors of this book have succeeded in gathering a series of outstanding short studies and in fulfilling the premise of the book's title.

PETER PASTOR
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GEORGE TADEUSZ ŁUKOWSKI. *The Szlachta and the Confederacy of Radom, 1764-1767/68: A Study of the Polish Nobility*. (Antemurale, number 21.) Rome: Institutum Historicum Polonicum Romae. 1977. Pp. 291. \$14.00

The book under review is a case study in political irresponsibility or, to borrow an expression from Herbert H. Kaplan's *The First Partition of Poland* (1962), "the dementia of Polish politics." In describing the causes and nature of the Confederacy of Radom, George Tadeusz Łukowski presents a vivid picture of the sad condition of the Polish Commonwealth on the eve of the First Partition.

The Confederacy of Radom was part of an ambitious Russian attempt to mobilize the gentry of the commonwealth in order to strengthen its own influence over the hapless state. Because of overwhelming military superiority the Russians succeeded; but, most amazing of all, many of the gentry willingly, even enthusiastically, participated in this venture of calling for the intervention of Catherine II in Polish affairs—before realizing, too late, that they were but pawns in a game being played by a master, for objectives quite alien to their own.

Any discussion of this event, which was so directly connected with the eventual partitions of Poland, must take into account the internal weaknesses of the commonwealth, as well as the pernicious role played by Prussia and, especially, Russia. Such discussions, particularly by Western observers, have tended to emphasize the "anarchic" conduct of the Poles themselves, especially of the self-willed magnates and the ignorant, reactionary, and xenophobic rank-and-file of the gentry. One of the major intentions of Łukowski's monograph is to "shed more light on the attitudes and mentality of the Polish nobility" (p. 10) and thus, in effect, to investigate the culpability of the Poles for their own ultimate plight, the loss of national independence.

In investigating his subject, Łukowski makes a valiant attempt at quantification, having compiled the names of the thousands of noblemen who signed the various local acts of confederacy between 1764 and 1767 and by using a computer to ascertain some correlations among the signers. Although maintaining the reservation that the available data were incomplete and somewhat unreliable, Łukowski nonetheless is able to reach several conclusions about the gentry. The most important are that family solidarity played a leading role in the formation of "political parties" and issues and that "the family was indeed a major political unit to be reckoned with at the local level" (p. 138). Łukowski also reaches the conclusion that as much as one-third or even one-half of the adult male *szlachta* population (out of a total *szlachta* population of approximately 950,000) may have belonged to the Confederacy of Radom, either directly or through the signature of a representative. "How many of them believed in, or even knew, what the Confederacy stood for, when its own leaders were none too sure, is another, unquantifiable, matter . . ." (p. 156).

The monograph also contains a number of interesting, sometimes provocative observations, for instance that "the Commonwealth was a secular theocracy" (p. 107), in the sense that "the Commonwealth and its Catholicism were indivisible" (p. 118), and that "equality was a totally bankrupt aspect of *szlachta* ideology, kept alive by the magnates, who saw in it the means of mobilizing the masses of petty nobility" (p. 118). This well-documented study, based upon a large number of archival materials, represents a genuine contribution to the historiography of eighteenth-century Poland.

HARRY E. DEMBKOWSKI
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JERZY MICHAŁSKI, editor. *Historia nauki polskiej*. Tom III, 1795-1862 [History of Polish Science. Volume 3, 1795-1862]. Wrocław: Ossolineum. 1977. Pp. xxv, 869. 240 Zł.

This is the third volume in a series entitled *Historia nauki polskiej* and the first of two projected volumes dealing with the development of Polish education in the "period of bondage, 1795-1918." The present volume deals with the stateless existence of the Polish nation in the years 1795-1862 and with that nation's intellectual progress despite the numerous adversities encountered under foreign domination. The volume begins with the assumption that Polish learning in this period lay on the periphery of European civilization and that it entered the main-

stream sometime after 1862. Nevertheless, the Poles, a nation with an ancient, rich, and diverse intellectual tradition, made considerable progress in the years 1795-1862, as individuals and institutions of education struggled to survive. Service to a politically divided Polish nation represented the common bond shared by the relatively small number of intellectuals, scattered throughout the partitioned Polish lands and in exile but persistent in their efforts in all fields of learning. The most notable intellectual developments in this period of service to the divided Polish nation were in the disciplines of history and language.

Among the book's many strong points is its organization. The volume consists of forty-four chapters divided into three parts, with an introduction by the editor of the series and contributions by many Polish scholars associated with the Institute of the History of Education, Science, and Technology of the Polish Academy of Sciences. The twenty-one chapters of part one introduce the reader to the historical circumstances under which Polish education developed. The two chapters of part two focus on the philosophical foundations of Polish education between the era of the Enlightenment and the era of positivism. The twenty-one chapters of part three deal with the development of specific disciplines. Each chapter represents the work of individual contributors. Chapters on the exact sciences, the natural sciences, rural economics, technology, historiography, and literature are included. In general, the individual chapters are clearly written and bring together valuable information concerning virtually all areas of education. One need not agree or concur with the individual interpretations to appreciate the value of this work. The result is a comprehensive synthesis of Polish education between 1795 and 1862 tempered by significant attention to details.

No undertaking of this scope, however, is without its problems—some avoidable nevertheless. Documentation is scanty. The omission of footnotes in a large work may be editorially justifiable, but long direct quotations (for example, pp. vii, 182, and 223-26) without documentation are disconcerting. The volume also lacks a bibliography, and the omission on page 244 of lines 9, 24, 27, 32, and 34 is a careless one.

Nonetheless, this impressive volume will serve as an indispensable compendium for students of *polonistyka*. It is regrettable that those lacking a knowledge of Polish will not find its information accessible elsewhere. The work deserves a wider audience and should not be relegated only to specialized libraries or to large university libraries with special Slavic collections.

JOAN S. SKURNOWICZ
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J. K. ZAWODNY. *Nothing but Honour: The Story of the Warsaw Uprising, 1944*. (Hoover Institution Publication, number 183.) Stanford: Hoover Institution Press. 1978. Pp. 328. \$12.95.

Historians deal very much in legend: they refute legends, they perpetuate them, and they create them. And, although the positivism that dominates our guild disdains legend, there is much evidence that the public enjoys it, needs it, and often looks to the historian to provide it. This can mean that, in certain situations, an author's sense of obligation may be painfully torn between the needs of a certain legend-seeking public and the demands of his profession. Such is the case with this book.

J. K. Zawodny examines the Warsaw Uprising and its implications at three levels, local, national, and international. At the local level, Zawodny draws on his own experience as a participant in the defense of Old Town as well as on interviews with other survivors to vividly describe that struggle. Other chapters analyze the nature of the AK (*Armia Krajowa*) forces that carried out the uprising, the character of the German response, and the impact of the uprising on the city and population of Warsaw.

Because of the large number of works already published on the local and international aspects of the uprising, it is in the area of its nationalist implications that Zawodny had the greatest opportunity to make a really fresh contribution to this topic. Due to the fiercely oppressive nature of the German occupation, authority in the Polish nationalist resistance movement was divided between the government-in-exile in London, an underground government in Warsaw, and the AK. According to Zawodny's account, the uprising exposed both the strengths and weaknesses in the relations and coordination between these groups.

As one would expect, given Zawodny's expertise in the field of international relations—he is Avery Professor of International Relations at the Claremont Graduate School and the author of a number of books on international relations and conflict—it is this aspect of the uprising that captures the greatest portion of his attention. After the Teheran Conference, to which Zawodny devotes two distinct chapters, nationalist Polish hopes of Western assistance in recovering Poland's 1939 frontiers began to ebb rapidly. With the formation of the Lublin Committee in the summer of 1944, the danger of the creation of a sovietized rump Poland began to loom in the eastern sky. Thus, although the uprising was fought against the Germans, its real target was Russia. Expecting the Russian summer offensive to bring the Soviet Army into Warsaw within a few days, the AK executed a

hastily planned uprising to insure that the Russians would be welcomed by an already existent independent Polish government. The failure of the Soviet Army to reach Warsaw left the insurgents in scattered positions throughout the city. Poland's Western allies then felt morally obligated to assist by means of airlifted supplies, while Stalin, who well understood the Polish objective, thwarted these efforts. The result was not only the failure of the uprising, but also the first major confrontation within the Grand Alliance over a matter related to the shape of postwar Europe.

Zawodny has taken pains to be objective in his study, even at the expense of Polish reputations. He places part of the blame for the insurgents' lack of ammunition on organizational failure. He also reveals the Bonapartist tendencies within the AK leadership, along with the internal wrangling within the government-in-exile. Nevertheless, the main theme of the book is that Poland was betrayed, so that, despite Zawodny's efforts at objectivity, his book is peopled chiefly with heroic Poles, deceitful Russians, atrocious Germans, hypocritical British, and ineffective Americans.

In an interview with Zawodny, Tadeusz Bor-Komorowski, the wartime commander of the AK, argued that the significance of the uprising could not be limited to its immediate military, political, or diplomatic repercussions. It also "brought forth cultural and moral values and strength that could not have come from a passive attitude. We cannot know at present what influence the AK's struggle will have on the formation of the Polish nation's spirit and positive values" (p. 215). Bor may have been correct that the ultimate significance of the uprising will be in its value as a national legend from which future Poles may draw inspiration and an increased sense of national existence. This seems to be the message in the title of the book. Legends are national treasures, and it is perhaps within the framework of the need to maintain such a legend that this book should be most properly appreciated.

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A. L. SHAPIRO. *Problemy sotsial'no-ekonomicheskoi istorii Rusi XIV-XVI vv.* [Problems in the Socioeconomic History of Rus' of the Fourteenth to Sixteenth Centuries]. Leningrad: Izdatel'stvo Leningradskogo universiteta. 1977. Pp. 214. 1 r. 10 k.

With his latest volume A. L. Shapiro moves beyond his earlier studies on the Novgorodian land cadastres and joins the continuing controversy among Soviet historians over the emergence of nascent capitalism and the socioeconomic pre-

conditions for the rise of the sixteenth-century centralized Russian state. He does this by synthesizing Soviet research on three broad problems from the fourteenth through the sixteenth centuries: farming economies with particular attention devoted to the three-field system; manufacture, markets, and the role of the towns and hired labor in the economy; and the economic differentiation of the peasantry.

Shapiro marshals a number of important and subtle arguments, which draw distinctions between Western European and Russian economic developments and place quasicapitalistic trends in Russia in the larger context of a feudal society. Thus, although he accepts much of the research of S. G. Strumilin and D. P. Makovskii, he rejects N. E. Nosov's more exaggerated claims of protobourgeois economic relations in the sixteenth century by stressing that all positive economic "tendencies," such as money exchange and the use of hired labor, occurred in a society that increasingly used serfs and slaves.

Shapiro, nevertheless, does see several major changes in the Russian economy that facilitated the rise of the Muscovite state. Among these changes is the transformation of the fallow grain system to the three-field system. The key to the success of the three-field system was the introduction of manure fertilization and the *sokha* with board (*politsa*). Shapiro recognizes that the shifting fallow system continued until the late fifteenth century and that the slash-burn method (*podseka*) was widespread, but he believes the three-field system predominated in the sixteenth century. Shapiro does not adequately resolve, however, the difficulties over the interpretation of source references to a third field or three-field layout. Nor does he draw any necessary connection between fallow systems and large lay or ecclesiastical estates with an enserfed peasantry. Difficulties also abound by tying specific tools to agrarian systems.

Despite the Mongol invasions, Shapiro believes an urban revival occurred from the second half of the fourteenth century through the 1570s, when war, Ivan the Terrible's *oprichnina*, famine, plague, and the Time of Troubles undermined the urban economy. Urban markets, money exchange, accumulation of merchant capital, and simple forms of manufacturing cooperatives with hired labor were all necessary preconditions for changes in the state structure, but Shapiro argues that these were not the rudiments of a bourgeois economy. Instead, these changes occurred within a "political and economic feudal state," which hindered any genuine capitalism from taking root.

Lastly Shapiro discerns little economic differentiation among the peasantry until the sixteenth

century when the urban markets made inroads into the countryside. But such economic differentiation pales before the growth of serfdom and slavery. In many ways, Shapiro stretches to the limit his Marxist framework, and in so doing reflects both the strengths and weaknesses of Soviet scholarship.

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O. N. VILKOV *et al.*, editors. *Istoriia gorodov Sibiri dosovetskogo perioda (XVII-nachalo XX v.)* [The History of the Towns of Siberia in the Pre-Soviet Period (Seventeenth to Early Twentieth Centuries)]. Novosibirsk: Izdatel'stvo "Nauka," Sibirskoe otdelenie. 1977. Pp. 301. 99 k.

M. M. GROMYKO and N. A. MINENKO, editors. *Iz istorii sem'i i byta sibirskogo krest'ianstva v XVII-nachale XX v.: Sbornik nauchnykh trudov* [On the History of the Family and Daily Life of the Siberian Peasantry from the Seventeenth to the Early Twentieth Centuries: A Collection of Scholarly Works]. Novosibirsk: Novosibirskii gosudarstvennyi universitet. 1975. Pp. 158. 70 k.

The publication of these two works clearly indicates the continued Soviet interest in the history of the vast area known as Siberia. Unfortunately, the two collections also continue two characteristics of Soviet treatment of that history, that is, the unwillingness to set the article-length study into historical perspective and a marked tendency to stay away from the broader questions. As the introduction to the first work admits, the contents of the book are diverse. The same description could be applied to the second publication. Together they contain twenty-five articles in 459 small pages from twenty-four scholars (one author has an article in each book). The length ranges from a high of thirty-nine pages to a low of ten.

Istoriia gorodov Sibiri dosovetskogo perioda, which is a companion volume to *Goroda Sibiri* [The Cities of Siberia] (1974), is divided into four sections: sources and historiography, general problems of history and administration, social-economic history, and the history of the construction and architecture of selected Siberian cities. Within those divisions the articles treat a number of topics relevant to a single city or a more general theme that touches upon several cities. Almost all of the articles are in part or extensively based on archival sources from the central archives and, more interestingly, regional ones in such cities as Omsk, Kurgan, Tomsk, and Tobolsk. Too many, however, lack a general conclusion or even an attempt to place the topic in any kind of historical per-

spective. That does not mean the collection is without value.

V. V. Rabtsevich's work on the social composition of the organs of city self-government brought by the reforms of 1785 illustrates well the problems of implementing the reforms in Siberian cities. N. A. Minenko, the only author to contribute to both collections, makes a useful examination of the urban family of Western Siberia at the end of the seventeenth and beginning of the eighteenth centuries. G. A. Khristosenko's study of the colonization of Nerchinsk up to the first half of the eighteenth century is well researched and useful. One minor theme does appear in some of the articles concerned with economic history. Several explore the period when the city under consideration developed from a military-administrative center, which most of them started as, to a trade center. Neither the authors nor the introduction make mention of this theme. The introductory remarks provide a useful bibliographical review of what has been published in Russian since the appearance of the earlier companion volume on Siberian cities.

Iz istorii sem'i i byla sibirskogo krest'ianstva has more internal consistency. Five of the six articles deal with the life styles and world views of various groups of the Siberian peasantry. For example, M. M. Gromyko attempts to reconstruct the pre-Christian beliefs of the Siberian peasantry at the end of the eighteenth and the beginning of the nineteenth centuries by analyzing the content of peasant ceremonies as well as some magical practices. L. V. Ostrovskaia examines the peasants' view of selected aspects of folk medicine. The peasant groups examined are defined by geography or by time period. Except for the subject matter treated and the fact that the methodological approach tends to be ethnographic or folkloric, the five articles contain no connecting theme. The work by N. A. Minenko stands apart from the rest in this collection. Minenko's study examines the size and the structure of the peasant family of Western Siberia in the first half of the nineteenth century. The collection lacks both an introduction and a conclusion.

The authors of the introduction to the first work, O. N. Vil'kov and D. Ia. Rezun, argue that one of the end goals of Soviet historical research is generalization. Short of this, however, they add that there are critical and little-studied questions in the history of Siberian cities. In their opinion scholars should examine the economic development of the cities, the social composition of the population, and, of course, the ever present "development of the class struggle." If we add to their list of basic short-term goals the amassing of information about the beliefs and life styles of the Siberian

peasantry, then these two books make a definite contribution despite the absence of generalization.

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RAYMOND H. FISHER. *Bering's Voyages: Whither and Why*. Seattle: University of Washington Press. 1977. Pp. xiii, 217. \$17.95.

In the summer of 1728 Vitus Bering, a Dane serving in the Russian Imperial Navy, sailed through the North Pacific strait that now bears his name. In 1741 Bering and Aleksei I. Chirikov, who captained a second vessel, discovered the Northwest coast of America and established Russia's claim to Alaska. Most historians have viewed the purpose of these voyages as an attempt to answer two geographical questions: were the continents of America and Asia joined by land and was there a North-east Passage to the Far East? This thesis lately has been challenged by several Soviet scholars including A. A. Pokrovskii, V. I. Grekov, A. V. Efimov, and, most recently, B. P. Polevoi. They argue that economic and political purposes lay behind the expeditions. Peter the Great's instructions (1725) ordered Bering to visit America and these historians suggest that expansion of Russian trade and extension of Russian power in East Asia and the North Pacific were the true motives underlying the voyages.

Drawing on these scholars—particularly upon Polevoi—Raymond H. Fisher has concluded that Peter's purpose in sending Bering to the North Pacific was the expansion of Russian territory to the American continent. Fisher places Bering's expedition in the context of Peter's Siberian policy: "Having become a naval power, Russia need no longer look on the ocean as a barrier to continental eastward expansion. . . . Peter . . . had to know exactly where northwest America was situated in relation to Siberia and how close to Russia the possessions in America of the Western imperial powers were." Thus, Peter sent Ivan Evreinov and Fedor Luzhin to Kamchatka in 1719 to find out whether "America is joined with Asia." In 1722 Evreinov presented Peter with a map that showed no land connection between Asia and America. By 1724 Peter had at least two other maps in his possession that pictured the continents, from the Lena River in the Arctic to Kamchatka on the Pacific, separated by sea. Fisher argues that Bering could not have been sent by Peter to answer geographical questions that were settled previously. Rather, the tsar "sent Bering to find America."

Other writers have misread Bering's motives be-

cause they have been confused by the meaning of Peter's instructions to sail "along the land which goes to the north" which "is part of America." Fisher (along with Polevoi) demonstrates that the Homann map, upon which Peter most likely relied, shows land (sometimes called Juan de Gama Land) east of Kamchatka and unconnected to Asia. This land was thought to be part of America and, according to the Homann map, Bering could reach it by sailing northeast from Kamchatka. Not discovering Juan de Gama Land in 1728, Bering's second voyage attempted to locate it by sailing east in 1741. Of course, Juan de Gama Land was mythical, but in searching for it Chirikov and Bering stumbled upon Alaska.

Fisher shows that on both voyages Bering was instructed to keep his destination secret—if he met European ships or came to a European settlement in America, he was to state that his voyages were of a geographic and scientific nature. Reliance on these public instructions, faulty translations, and the failure to analyze the maps available to Peter and Bering have led other scholars to misinterpret the purpose of the voyages. Fisher is particularly critical of both the methods and the conclusions of Frank A. Golder, Robert J. Kerner, and the Soviet historian E. V. Kushnarev.

All this adds up to a convincing case, but a circumstantial one. Fisher warns that "the Soviet archives have yet to be mined for what they may or may not tell us on this subject." All conclusions, he admits, must remain "a matter of speculation and opinion, dependent upon indirect evidence." What is presented as speculative at the beginning of the work, however, often seems to take on an aura of fact as Fisher proceeds with his case. For instance, arguing for his interpretation of Peter's motives, Fisher relies upon "the Homann map that he [Bering] carried." Earlier the author admits that only "circumstantial evidence" supports the conclusion that Peter based his instructions on this map or that Bering carried it with him in 1728. Nevertheless, if one bears in mind that Fisher's conclusions are tentative, the work makes an important contribution to the history of Russian expansion. It remains for future scholars to test the ground that Fisher has broken.

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W. BRUCE LINCOLN. *Nicholas I: Emperor and Autocrat of All the Russias*. Bloomington: Indiana University Press. 1978. Pp. 424. \$15.95.

W. Bruce Lincoln's *Nicholas I: Emperor and Autocrat of All the Russias* is a major historical study. To

state its limitations first, it is largely based on secondary works and published documents; the author utilizes archival sources to develop certain points, but he brings no large new body of material into the ambit of scholarship. Also, the study projects no fundamentally new interpretation of Nicholas I or his reign, and thus it leaves our basic perception of that important period of Russian history unchanged—at least my own perception was not changed by the book. But once it is noted that *Nicholas I: Emperor and Autocrat of All the Russias* is not *The Civilization of the Renaissance in Italy*, what remains to be said is essentially praise. Lincoln's volume is the most balanced, reliable, and up-to-date treatment of its subject not only in English but in any language. It is also a thoroughly intelligent and learned investigation and presentation, marking as it does the culmination of many years of work in the field (fifteen articles by the author are listed in the bibliography). Its most valuable contribution may well be its discussion of the process of government in Russia at the time and of the governing personnel, the focus of much of Lincoln's previous research.

As to point of view and interpretation, different readers are likely to raise different questions. I am not fully convinced by the author's neat explanation that Nicholas I could not engage in major reform, because such reform would work against the interests of the gentry, and the emperor could find no other group to rely on to introduce reform, whereas Alexander II was free to act because he had a sufficient body of enlightened bureaucrats to support him. More broadly speaking, Lincoln seems to overstate somewhat the social well-being, integration, and unity in the reign of Nicholas I, which witnessed, in my opinion, the irreparable break between the more progressive intellectuals and the government. And one can cavil at such assertions as the one that it was with the suppression of the Decembrist rebellion that Nicholas I "came to see himself as the champion of order both at home and abroad" (p. 47): as the author himself so well demonstrates, the Russian autocrat never had nor was likely to have any other view of his role. On the whole, however, I am in such fundamental agreement with Lincoln's evaluation of Nicholas I and his activities, be it the intrinsically defensive nature of the foreign policy of the reign or the main aims the government pursued at home, that I make a poor critic of the author's interpretations—rather, other historians will have to criticize both of us.

The book is clearly written and attractively published. There are few misprints in the text, but, unfortunately, several dozen in the notes relegated to the back of the volume, the selected bibliogra-

phy, and the index, where insufficient attention has been paid to the Russian forms. In the inevitably vexing matter of translation and transliteration, the author has heavily emphasized direct rendition from the Russian with the result that a number of Western names (usually of non-Russians in the imperial Russian service, such as Baltic Germans) acquire a new spelling. There are also occasional curiosities, such as "François Baader" (pp. 105, 415). A striking portrait of Nicholas I gazes at the world from the dust jacket.

In conclusion, it is a pleasure to welcome this fine book. Its attractive qualities include its generosity to the earlier authors in the field who receive more credit than we deserve.

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STEPHEN T. COCHRANE. *The Collaboration of Nechaev, Ogarev and Bakunin in 1869: Nechaev's Early Years*. (Osteuropastudien der Hochschulen des Landes Hessen, series 2. Marburger Abhandlungen zur Geschichte und Kultur Osteuropas, number 18.) Giessen: Wilhelm Schmitz Verlag. 1977. Pp. xx, 365. DM 70.

This book takes up three worthwhile topics: (1) Nechaev's obscure youth; (2) his revolutionary experience in St. Petersburg in 1868-69; and (3) his first trip abroad in the spring and summer of 1869. Stephen T. Cochrane tells us that Nechaev's "childhood, adolescence, conversion to radical activities and the events of his first stay in Geneva have never been studied as a single, continuous development" (p. xv). That seems still the case.

Like many dissertations, the text of this book is a verdant jungle of information, but it is nearly trackless, guideless, and unexplorable. The narrative stumbles upon many rich resources, but very early the reader comes to distrust it and loses hope that he will find his way out with anything of value. Frequently word choice is misleading, sentences are only approximate gestures at clear sense, and few paragraphs achieve any sort of pointed lucidity. The overall concept of the book, from the wording of the title on, is tangled and fragmentary. There is precious little "single, continuous development."

The first chapter concludes that Nechaev grew up in a certain "atmosphere" (the word is used often, for example on pages 3, 21, and 31). If he had been "made of other stuff" (p. 22), he might have become a teacher, but for "deeply psychological" (but unexplored) reasons (p. 28), he became an internationally famous terrorist and deceiver. Less labored attention to textual detail and a more

imaginative and coherent exploration of Nechaev's "class consciousness" are needed at this point. The evidence exists, and most of it is cited (though Pomper's article in the bibliography seems not to have been used well, and Brower's book is not cited at all) to construct a fascinating story about Nechaev's native-born and nurtured class hatred.

The second and third chapters lead the reader past a series of quotes from Nechaev's, Bakunin's, and Ogarev's writings. Cochrane takes great pains to analyze significant statements of program and theory, identifying individual lexical differences that allow authorship of certain disputed texts to be argued plausibly.

The third chapter, about 80 percent of the book's narrative, is devoted specifically to "the collaboration" referred to in the title. The author slightly adjusts Confino's conclusions about the Bakunin-Nechaev relationship and does a good service to Ogarev by bringing him forward for separate attention. Ogarev has suffered long in the shadow of Herzen.

The greatest value of this book would have to be the lush, perhaps rank, citations and quotations from scarce publications. But the reader can never trust that what he is getting is what has been promised. On pages 52-55, for example, the author claims to have translated "in full" the "Program of Revolutionary Actions" because it is fundamental to an understanding of "the atmosphere" in which Nechaev's group worked and "the direction Nechaev took." Careful analysis of the translation reveals that words or phrases were missed or omitted on sixteen occasions; for example, the sentence which says that members must "give up their own affairs" should have read "give up their property and their private affairs."

In addition I noted twenty-four other errors in translation in this one document: for example, *ob'iasneniia* is translated as "unification"; *sledovatel'no obrazovanie organizatsii iz samoi gol'by* as "consequently, education by the poor themselves through the organization"; and *postavlen vlasti v nevozmozhnost' deistvovat'* as "would be jeopardized by the existing powers."

There is a good, reliable index. The book might satisfy certain reference needs but would have to be used with extreme patience and caution. If the editors of the series had been a bit more demanding, the author might eventually have shaped his work into something of real value.

ALAN KIMBALL
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ROBERT J. BRYM. *The Jewish Intelligentsia and Russian Marxism: A Sociological Study of Intellectual Radicalism*

and *Ideological Divergence*. New York: Schocken Books. 1978. Pp. viii, 157. \$16.95.

In his monograph, *The Jewish Intelligentsia and Russian Marxism*, Robert J. Brym presents a theory on the development of a radical intelligentsia and especially on its dependency on contemporary society for its own identification and ideological formulations. Brym's conclusions are based on his analysis of biographical and historical literature on 207 Jewish activists who joined one of four Marxist-based revolutionary parties within the Russian Empire: the Bolshevik, Menshevik, *Bund*, and *Poale Zion* parties. By his own admission, Brym does not produce any new information on these individuals. Rather, he seeks to reorganize the existing material so as to highlight the substantial relationship between the intellectual and his cultural milieu.

Although Brym's own theoretical approach, outlined in the first chapter of the work, stresses flexibility in its consideration of data, it becomes quite deterministic when practically applied. The key term for Brym is "embeddedness." He establishes a numerical coefficient indicating the degree to which his subjects were embedded in the Jewish community and its network of relationships. He concludes that Jewish Bolsheviks came from families that had become economically and culturally embedded in Russian society, while those radicals who were more deeply embedded within Jewish life later came to be classified as either Bundists or Poale Zionists. In Brym's schema, the influence of the working class plays a decisive role in the intellectual's final choice of identity and orientation. Hence, where a strong Jewish working class dominated local life, the radical would become a Bundist; while in those areas where a Jewish work force was absent and the Jewish bourgeoisie still held a powerful hold on the community, the individual would become a member of the *Poale Zion* party, that group most embedded in Jewish life. In this view, then, once the degree of embeddedness has been established, the individual's options became greatly reduced. Further movement and ideological development did take place but only within closely circumscribed parameters.

Brym's work is not without problems. He anticipates that critics will find individual exceptions to his typologies, but the most telling criticisms are the very real limitations of the theory itself when applied to other categories of intellectuals and the internal inconsistencies that emerge in the presentation of the theory. First of all, the usefulness of the approach must be questioned when it proves to be highly restricted in its application. Although Brym proclaims the general validity of the theory, application to contemporary Jewish non-Marxists

is problematic. A survey of prominent Jewish anarchists, populists, socialists, and even nihilists shows that such individuals came from families embedded in both Jewish and Russian culture with no apparent pattern of recruitment to any of those movements. Thus, examinations of the embeddedness of an Aaron Liberman, or an Abraham Kovner, or a Vladimir Jabotinsky, for that matter, give few clues as to their future intellectual stance.

As for the internal contradictions, Brym posits that because the modernizing agency for Jewish life was the Jewish Enlightenment, itself deeply indebted to the Western Enlightenment, Jews were much more attuned to Western ideas and values than were their Russian counterparts. Yet, in their doctrinal approach to certain questions, Poale Zionists came closest to that position championed by the Bolsheviks, the party most embedded in Russian culture. Thus, in such instances, contrary to Brym's view, embeddedness played no role in the determination of final perspective.

In conclusion, then, Brym does present interesting findings on the general character and geographical origins of some Russian-Jewish Marxists. Although his work is challenging and thought-provoking, however, his observations remain too sweeping to illuminate adequately a very complex and intricate set of relationships.

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B. V. KONDRIKOV. *Krest'ianskoe dvizhenie v Zapadnoi Sibiri v period pervoi russkoi revoliutsii* [The Peasant Movement in Western Siberia During the Revolution of 1905]. Omsk: Zapadno-Sibirskoe knizhnoe izdatel'stvo. Omskoe otdelenie. 1976. Pp. 158. 1 r.

Peasant rebellions contributed substantially to both of Russia's twentieth-century revolutions. Yet the study of the peasant movement in the First Russian Revolution remains in its infancy. Outside of a flurry of commemorative books, articles, and extremely valuable documentary collections published around the twentieth and fiftieth anniversaries of the 1905-07 Revolution, the Soviets have provided us with relatively little on the subject, while Western specialists on early twentieth-century Russia have shied away from such a complex and enormous subject as the peasantry since Geroid T. Robinson's out-of-date study, *Rural Russia Under the Old Regime*. Therefore, any new contribution to this relatively neglected area of inquiry like B. V. Kondrikov's *Krest'ianskoe dvizhenie v Zapadnoi Sibiri v period pervoi russkoi revoliutsii* is most welcome.

Although the first to study systematically the

peasant movement in Western Siberia in the 1905–07 period, this book is most disappointing, clearly not living up to the standards of the better Soviet works on the subject. The author maintains that despite “certain peculiarities,” namely a quite different social-economic climate in which to operate, the peasant movement in Western Siberia was virtually identical in character to that in central Russia. Such a thesis spares the author the necessity of interpreting his data, allowing him to apply Lenin’s often perceptive analysis of the 1905–07 peasant rebellions in central Russia to Western Siberia with scarcely any modifications. Unfortunately, this thesis is not supported by the statistical data presented in this work. Although the Western Siberian and central Russian peasant movements flowed and ebbed together in 1905, they departed from one another quite strikingly in 1906 and 1907. Nowhere in this work is this important discrepancy between the thesis and evidence ever presented or faced. Nor is there any discussion of the similarities and differences in the changing economic practices of the government-managed state lands and lands of the royal family, against which the Western Siberian peasant movement was primarily directed, and those of the gentry-owned estates, against which the huge bulk of the peasant disorders in central Russia were directed.

In addition, Kondrikov applies Lenin’s thesis of “proletarian hegemony” in the First Russian Revolution in a most heavy-handed fashion, identifying this phenomenon almost exclusively with the political influence and activities of the Russian Social Democratic Labor Party, especially its Bolshevik wing. Yet the work of the party in the countryside, as Kondrikov himself admits, was generally “sporadic” and limited in nature to the point that one cannot justify devoting a full half of a work on the Western Siberian peasant movement to this theme, especially when no attempt whatsoever has been made to assess the efficacy of Bolshevik activities in the villages, to compare these activities with those of other political parties, or to determine the precise relationship of these activities to the peasant movement. Were the villages reached by the Bolsheviks and other revolutionaries more likely to engage in disorders? If so, what kind?

Finally, nowhere in this work does the author explore the social psychology or social dynamics of the peasant movement or touch upon issues of methodology, especially the difficulties in determining precisely what constitutes “a peasant disorder.” Yet this problem has been recognized by increasing numbers of Soviet scholars on the peasantry in recent years, including the author of the introduction to this book, S. M. Samosudov. Moreover, by insisting—and rightly so, I think—

on the importance of passive as well as active forms of struggle (that is, tax boycotts, draft resistance, electoral behavior, demeanor in the presence of officials or estate owners), Kondrikov has rendered the problem of defining “a disorder” still more complicated. At the same time, he fails completely to question the efficacy of standard Soviet indexes for gauging peasant unrest—the number of disorders (however defined or not defined) and the proportion of counties affected—and does not venture to wonder whether the proportion of villages affected, the number of participants, the number of those arrested, or all of these together might not prove more reliable guides for measuring levels of peasant involvement in the revolutionary movement. Nonetheless, one cannot hold the author solely responsible for these deficiencies. No scholar of twentieth-century Russia—Soviet or American—has yet ventured to tackle these important and difficult questions.

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RALPH CARTER ELWOOD. *Roman Malinovsky: A Life Without a Cause*. (Russian Biography Series, number 2.) Newtonville, Mass.: Oriental Research Partners. 1977. Pp. 107. Cloth \$8.00, paper \$3.90.

Roman Malinovsky, like Azev, provides the history of the Russian revolutionary movement with intense melodrama and mystery. Born a Polish peasant, a drifter and thief in his youth, Malinovsky took up a highly successful career as a labor organizer, revolutionary, and politician. In 1912 Malinovsky became a member of the Bolshevik Central Committee and a member of the Fourth State Duma. In 1914 he resigned from the Duma and left the country amidst rumors that he was a double agent for the government, an Okhrana spy. Although exonerated in 1914 by a tribunal that included Lenin, Malinovsky was tried again in 1918 by the new Soviet government, found guilty, and shot.

The tangled web of fact and fiction surrounding Malinovsky’s career has been skillfully unraveled in this new short biography by Ralph Carter Elwood. The book is part of a series published by Oriental Research Partners, whose aim is “to present to the scholarly world the lives and activities of many of the leading personalities of the day who have unfortunately slipped into complete obscurity.” Through Malinovsky’s story, the book tells us more about the police than the revolutionary movement. Elwood ably presents and supports the novel thesis that Malinovsky’s resignation from the Duma was his last act of provocation for the Okhrana “because this would discredit the revita-

lized Bolshevik leadership in the eyes of both the party rank and file and the resurgent masses which were following them." Elwood writes well, has thoroughly researched his subject, and the resulting story has all of the gripping excitement of a paperback thriller.

Yet one is inclined in the end to wonder whether Malinovsky's obscurity is truly "unwarranted," as Elwood claims. Malinovsky had no discernible impact on the Bolshevik movement or the Soviet state. Though his story throws some light on both the Okhrana's methods and Lenin's faith in his followers, it does little to magnify the historical importance of Malinovsky himself.

GEORGE D. JACKSON
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SHEILA FITZPATRICK, editor. *Cultural Revolution in Russia, 1928-1931*. (Studies of the Russian Institute, Columbia University.) Bloomington: Indiana University Press, 1978. Pp. vi, 309. \$17.50.

That the Chinese Cultural Revolution of the 1960s had a Soviet precedent during the turbulent years of the First Five-Year Plan is a fact that has been ignored by Soviet historians and insufficiently appreciated by Westerners. This collection of essays, the product of a conference held at Columbia University, should help to remedy the situation, at least in the West. In the introductory chapter, which provides an original synthesis of the topic as a whole, editor Sheila Fitzpatrick defines this type of cultural revolution as proletarian class war against "bourgeois" influences accompanied by official encouragement for visions of radical transformation in virtually all spheres of society and culture. The rest of the volume contains more narrowly focused papers by Moshe Lewin on the transformation of society, Gail Lapidus on education, David Joravsky on psychology and neuropsychology, Susan Solomon on rural sociology, George Enteen on historical studies, Robert Sharlet on law, Katerina Clark on literature, S. Frederick Starr on town planning, and a concluding essay by Jerry Hough on the implications of the new findings for our understanding of the Soviet system under Stalin.

The most important general propositions that emerge from these pages are as follows: that the 1928-31 period witnessed a radical utopianism in the sociocultural sphere that sharply differentiated it from both the New Economic Policy that preceded it and the decade of the 1930s that followed; that this cultural revolution consisted of not only the persecution of bourgeois specialists and intellectuals by doctrinaire Communists but also a genuine and dramatic increase in upward mobility

by members of the working class; that the forces of cultural revolution were not uniformly initiated or controlled from above, but arose to a significant degree from the social conflicts of NEP and the frustrations of middle- and lower-level Communists; that the end of the period marked the end of efforts to transform culture and society according to ideological blueprints, as the Communist zealots who had waged the cultural revolution fell by the wayside, the old intelligentsia was welcomed back into positions of status, and the newly promoted proletarian cadres became increasingly bourgeoisified in taste and outlook; and, finally, that this reinterpretation of both the 1928-31 period and its aftermath serves to discredit the totalitarian model usually applied to Stalinist Russia.

Not every contributor would agree with all of these propositions, since they occasioned much debate at the conference and, in fact, primarily bear the stamp of Fitzpatrick and Hough. Fitzpatrick has performed a valuable service by pointing out the extent to which the Komsomol, rank-and-file party members, and nonparty workers had long demanded the right to conduct the purging of "bourgeois" elements that became the moving force of the cultural revolution. But care must be taken in labeling this movement as "popular" or a "revolution from below" or in implying that it forced the hand of the leadership. If anything was popular at this time it was the desire of the peasantry (some 80 percent of the population) for a continuation of NEP policies, but this fact did not prevent Stalin from pursuing the opposite course. There were many countervailing forces in NEP Russia, and the specific contours of both the cultural revolution and the First Five-Year Plan were the definite if not always anticipated result of high-level decisions concerning which forces to "unleash" (in Voronsky's phrase), which to keep chained, and which to bludgeon into submission.

Hough and several other contributors uncritically accept Timasheff's term, "the Great Retreat," as a description of the ending of cultural revolution in the early 1930s. The word retreat implies a return to ground previously held, but the new system welded together disparate elements, some new, some old, which made it significantly different from all those that had preceded it, including tsarist authoritarianism. The restoration of conservative social forms marked a startling reversal of both the radical utopianism of the cultural revolution and the moderate experimentalism of NEP. But what gave mature Stalinism its distinctive nature was the combination of this new social conservatism with the continuation and intensification of other traits that had emerged during the First Five-Year Plan: the dynamism needed to mobilize the entire population in the

pursuit of political and economic goals; the determination to snuff out autonomy and to achieve control over all aspects of individual thought and activity; the subsuming of all intellectual and cultural issues within an all-embracing ideology of which the party leadership served as final arbiter. By overlooking these elements of continuity, Hough has exaggerated the importance of the discontinuities between the First and Second Five-Year Plans, thereby overemphasizing the differences in the Soviet system during these two periods.

Some of the interpretations, therefore, should be treated with caution. But this fact in no sense detracts from the value of this stimulating work, which has clearly achieved its purpose of defining vital new issues for further scholarly research and debate.

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LEV KOPELEV. *Khranit' vechno* [Preserve Forever]. Ann Arbor: Ardis. 1975. Pp. 732.

LEV KOPELEV. *To Be Preserved Forever*. Translated and edited by ANTHONY AUSTIN. Foreword by LILLIAN HELLMAN. Philadelphia: J. B. Lippincott. 1977. Pp. 268. \$12.50.

Here is the original of Solzhenitsyn's Rubin, the Soviet Communist as tragic hero and victim, speaking for himself. He is older and wiser than he was in *The First Circle* but still much more vivid and believable than Nerzhin, the fictional persona in which Solzhenitsyn presented himself. Nerzhin-Solzhenitsyn was the political prisoner of comic-strip purity, who refused to cooperate, at the cost of banishment from a *sharashka* (a prison research institute) to a hard-labor camp. Rubin-Kopelev was Stalin's faithful servant Job, arguing with the faithless that the grand cause merited grand sacrifices, including jailings on mere suspicion of heresy—yet sleepless with doubts and memories, for example, of taking grain from starving peasants and forcing them into collective farms. Now the real Lev Kopelev reveals that he was even an informant for the political police in the 1930s. When he wrote those reports he was proud that he told no lies, but now he sees no essential difference between a "fantasizing stool pigeon" and a "realistic stool pigeon" (Russian, p. 279; English, p. 112). At that point he quotes again the line of Pushkin's that stands as epigraph to the confessional part of the book: "bitter tears I shed,/But don't wash out the lines of sorrow."

Anna Akhmatova's pledge is epigraph to the

other part of the book: "For them I will weave a memorial shroud/of words, of their own poor words that I heard." (I have changed Austin's translation of the epigraphs, to get closer to the originals.) Kopelev as witness has a keen ear, which one might expect of a masterful linguist and literary scholar, and for once the scholar's ear and the writer's hand are harmoniously linked. Stalin's Russia emerges in its vivid particularity in dozens of life stories told with the distinctive rhythms, clichés, and occasional startling inventions of individuals choosing to be scoundrels or victims—or both, with an occasional gleam of the heroic. Kopelev's broad painting is far more particular than Solzhenitsyn's *Gulag*, which submerges individuals in the torrent of the author's raging self-expression. The difference is most easily seen in the portrayal of the genuine criminals who usually lorded it over the political in the camps. Solzhenitsyn presents "not faces," but "cruel, loathsome snouts, . . . gorilloids, . . . not people" (*Gulag*, I, pp. 501–02). Kopelev gives us an absorbing gallery of individual rogues. His dominant mood in painting them is rather Chekhovian, as it is in portraying himself and his fellow political; all are pathetic victims of a system, a social process, that none could comprehend or control. He even manages to extend that attitude—shall we call it Marxist or Christian rather than Chekhovian?—to repulsive individuals who fattened on the Stalinist system, including the regional party secretary, who picked a complaisant wife out of the personnel file and drove zealous Kopelev to jail for making other Communists feel morally uneasy and intellectually inferior.

The intellectual Kopelev (*intelligent* in the old sense of the term) clashed continually with the *vydvizhentsy*, workers and peasants "pushed up" into positions of authority. He kept trying to persuade them that he was on their side, but they refused to agree, insisting that he was corrupted by bourgeois humanism. In retrospect he finally agrees with his tormentors, lapsing thus into conscious heresy: humanism, whether bourgeois or socialist, is indeed at odds with the basic Soviet principles of *partiinost'* and *gosudarstvennost'*, the subordination of all considerations to the current interests of party and state as interpreted by their current masters. One can only guess how widespread such alienation is among Soviet intellectuals. If Kopelev is typical, Soviet leaders have undermined their own legitimacy by insisting too furiously that everyone must kowtow to it.

Space does not permit adequate discussion here of this important book. I hope the publishers will decide on a new complete translation. Two-thirds of the book have been cut away in an insulting effort to increase the book's appeal. The surviving

third has been rearranged to fit its episodes within a simple-minded chronology. Thus, Kopelev's strenuous effort at ruthless self-discovery, by repeated flashbacks and flashforwards that instantly demolish rationalizations and excuses, is greatly weakened. And his language is persistently watered down. Only a couple of examples are possible. "Servile functionaries of death" becomes "callous bureaucrats" (Russian, p. 44; English, p. 99). Brooding over his youthful Stalinism, Kopelev remarks: "The great cause is words, suitable words, signifying many things and if necessary signifying nothing at all, but explaining everything" (Russian, p. 29). That is chopped down to "Words! What can they not explain away when you want them to!" (English, p. 93).

In a new, complete translation space can be saved by dropping the Hollywood touch that Lillian Hellman gave the present edition. She wrote a gushy "Foreword" to a book she did not read.

DAVID JORAVSKY
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ALEKSANDR M. NEKRICH. *The Punished Peoples: The Deportation and Fate of Soviet Minorities at the End of the Second World War*. Translated by GEORGE SAUNDERS. New York: W. W. Norton, 1978. Pp. xii, 238. \$10.95.

The Soviet historian Aleksandr M. Nekrich attracted attention in the West when his frank and fresh study of the German invasion of the Soviet Union (1941-22 iunia [1965]) was published—and damned—in Moscow. It appeared at a time when his thesis, making Stalin responsible for Soviet setbacks early in the war, had again become impermissible. Nekrich was bitterly assailed and ostracized. When he emigrated from the USSR in 1976, he brought with him the draft of another manuscript, dealing with the fate of those non-Slavic nationalities whose autonomous areas Stalin abolished during the Second World War and whose populations were deported eastward and only partially "rehabilitated" after Stalin.

This dramatic story can be read at many levels: as a series of case studies of people caught between German occupiers and Stalinist rule; as an interesting example of how Stalin's personality traits—his arbitrariness, his "fascination with treason and betrayal"—informed his policy decisions; or as an exhibit in the sad resort to "categorical" condemnation of entire populations without establishing individual guilt. The whole story is a curious example of the suppression, manipulation, and falsification of information—from evidence regarding popular attitudes on the part of the Crimean Tatars, Kalmyks, Chechens and Ingush, Balkars,

and Karachai (all formerly Moslem or Buddhist ethnic groups) to the secrecy surrounding the abolition of their regions and republics and the exile and resettlement of their populations. Nekrich gives some examples of downright falsifications found in Soviet versions of these peoples' histories to this very day. He provides rich information on differences in attitudes and behavior among Soviet nationalities, including instances of bitter hostility between local Russian settlers and returning exiles in recent years. He also offers valuable data on the role of local party officials in discriminating against natives in these areas and in reporting to Moscow during the war about the "collaborationist" inclinations among, say, Crimean Tatars and Kalmyks—reports that may have prompted Stalin's decision to "liquidate" the problem by writing off entire nationalities as collaborators with the enemy.

Although the outlines of the story have been told before, Nekrich's is a meticulous account, using a variety of sources, a number of which were not previously available or known abroad. These include Soviet dissertations, interviews with former residents of the areas concerned, and unpublished manuscripts and memoirs. Thus, it is a far richer account than earlier studies. At the same time, the author is by no means uncritical of his sources (for example, identifying certain recent German books as apologias for wartime behavior). Nekrich carefully examines the evidence on the extent to which the population of any of these national areas did in fact collaborate with the enemy and rightly finds the allegations vastly exaggerated. He also describes the postwar fate of these groups and their strenuous, at times tragic, efforts to revert to full citizenship status back home. The author is forced to conclude that "we must view the deportation of the peoples of the Caucasus and the Crimea, the Kalmyks, and in part the Baltic peoples not only as an expression of the arbitrary lawlessness of the Stalin regime, which of course it was, but also, and more importantly, as an inescapable part of the history of the Soviet state and of Soviet society and its intellectual culture" (p. 103).

Originally written for the Soviet reader, *The Punished Peoples* still shows traces of this orientation and intended audience. Moreover, Nekrich has produced something of a mix between documentation and solid, careful historical research, with understandable elements of emotion, morality, political argument, and a call for justice. Once again, the author stands revealed as a courageous scholar not afraid to speak out on sensitive issues. This work shows some signs of haste, suffers from the absence of maps, and leaves questions unanswered (for example, why were the Volga Germans left out of consideration?). Despite these short-

comings, Nekrich offers the general reader materials of historical value and interest, and he provides the specialist with some previously unknown references and information as well as with some novel hypotheses.

ALEXANDER DALLIN
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PAUL ROBERT MAGOCSI. *The Shaping of a National Identity: Subcarpathian Rus', 1848-1948*. (Harvard Ukrainian Series.) Cambridge: Harvard University Press. 1978. xiii, 640. \$25.00.

The sparse scholarly literature in English on the Subcarpathian Rusyns (Ruthenians) has been considerably enriched by this useful book. Half the volume surveys the national development of the Rusyns from the eighteenth century to their incorporation into the Soviet Ukraine after the Second World War. The other half consists of several appendixes: a discussion of the term, Subcarpathian Rusyn; eighty-one biographical sketches of Rusyn leaders who were active in the period after 1918; and specimens of the written languages used by the Rusyns. There is also a comprehensive bibliography of Rusyn studies containing nearly 2300 items.

The Rusyn movement in the nineteenth century shared many of the characteristics common to national reawakenings throughout Eastern Europe. Paul Robert Magocsi emphasizes the role of the Church as a cultural institution and the indispensable leadership exercised by the intellectuals, the majority of whom were priests. The works of history and philology produced by these intellectuals were the typical literary products of East European national movements. In many ways, then, the Rusyn experience was similar to that of the Rumanians, Slovaks, and Serbs of Hungary. Yet there were also fundamental differences, and these form the main subject matter of Magocsi's investigation. He presents abundant evidence to show why the process of national consolidation among the Rusyns took so much longer than among their neighbors. As causes he cites the pro-Hungarian orientation of the dominant Greek Catholic Church and its higher clergy, who worked for assimilation with Magyar culture, and the inability of clerical and lay intellectuals to agree upon the historical and ethnic identity of their people and a national language. At each important stage in the development of the Rusyns—the nineteenth century, 1918-19, the interwar period, and the Second World War—four currents competed for allegiance, the Rusyn, Magyarone, Ukrainian, and Russian. Magocsi describes each in detail and concludes that the intellectuals, who were so

sharply divided among themselves, could never get control of their people's destiny long enough to direct it along a single path of development.

The discussion of these problems is divided into three parts. The first deals with the nineteenth century through the incorporation of Subcarpathian Rus' into Czechoslovakia at the end of the First World War. The description of the various cultural currents among the Rusyns is well done, but perhaps more attention could have been given to political and economic problems. The reader familiar only with Western languages may supplement Magocsi's account with Ivan Žeguc's *Die Nationalpolitischen Bestrebungen der Karpato-Ruthenen, 1848-1914* (1965). Neither Magocsi nor Žeguc have used the rich materials available in the Hungarian National Archives in Budapest. Much is to be found there on the political, cultural, and economic policies of the Hungarian government toward the Rusyns, the Rusyn movement itself, and Rusyn emigration to and activities in the United States.

Parts two and three deal with the Rusyns in interwar Czechoslovakia. The chapters on cultural development present with admirable clarity the efforts of the various groups of Rusyn intellectuals to solve long-standing problems of ideology and ethnic identity. In Magocsi's opinion, Czechoslovakia provided the Rusyns with the most liberal conditions for development they had ever had, but, paradoxically, by 1938 government policies had succeeded in alienating the majority of them. In a sense, the Czechoslovak state may have been too liberal, for it fostered diversity rather than unity in Rusyn national life. The same cannot be said of the Soviet regime that installed itself in Subcarpathian Rus' in 1944. Magocsi's final chapter describes the "Ukrainian solution" imposed by the Soviet government, which for the majority of the Rusyns brought to a close the shaping of their national identity.

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NEAR EAST

FRANZ BABINGER. *Mehmed the Conqueror and His Time*. Translated by RALPH MANHEIM. Edited, with a preface, by WILLIAM C. HICKMAN. (Bollingen Series, number 96.) Princeton: Princeton University Press. 1978. Pp. xx, 549. \$30.00.

There is no doubt whatsoever that Mehmed the Conqueror was one of the great figures in world history. Franz Babinger's book, which first appeared in 1953, is generally regarded as the stan-

dard work on the subject. The long-awaited English version of the book has finally appeared, almost a quarter century after the original publication in German. Prepared by Ralph Manheim and approved by the author himself, this translation is based on the second German edition of 1959 and includes corrections and supplements introduced in the second Italian edition. A second volume containing sources and references, which Babinger had intended to prepare, never came out due to his untimely death. Thus, the English version, edited by William C. Hickman, gains importance in comparison with other editions as the only version that contains notes. These notes demonstrate Hickman's mastery of international scholarship on the subject since the first printing of the book.

Divided into seven books, the work gives a detailed account of political events from 1432 to 1481, centering on the personality of the conqueror. Book one, covering the conqueror's early life, is a summary of Babinger's previously published monographs and articles. Babinger is rightly skeptical of Oscar Halecki's hypothesis that Ladislas never ratified the peace treaty at Szegedin in 1444. Thanks to the details provided in a newly discovered Ottoman source, *Gazavat —i Sultan Murad b. Mehmed Han* (1978), we can now assert that the king confirmed by oath his acceptance of the Szegedin treaty. In view of the irrefutable evidence published in my book, *Fatih Devri Üzerinde Tetkikler ve vesikalar* (1954), we should discard the notion that "Mehmed Çelebi (Mehmed II) merely became regent of the Rumelian territories" (Babinger, p. 31). It is known that in 1444 Murad II retreated into a dervish convent in Bursa before the battle of Varna and resumed his court life in Manisa only after the battle.

Book two deals with the period 1451–56, including the conquest of Istanbul and its reconstruction, as well as the campaigns in Serbia, the Morea, and on the Aegean Sea. Babinger does not give adequate attention to two points here. The significance of the internal rivalry between the war party and its opponents in the Ottoman government should not be ignored. This rivalry was as important as external events in the final decision to attack Istanbul in 1453. In addition, Mehmed's strategy and the use of cannon were key factors in his rapid conquests as contemporary observations by Tursun Beg and Kritovoulos make clear. Later editions should correct one small mistake in this chapter: "Hasan, commander of Corinth," (p. 125) was actually John Assan Zaccaria.

Book three, which covers the period from 1456 until the beginning of the Ottoman-Venetian War (1463), treats the conquests of the Black Sea coast and the Aegean area. The years 1463–64 consti-

tuted a turning point, since it was in these years, following Mehmed II's conquests of the Morea and Bosnia, that his two principal rivals, Venice and Hungary, finally joined forces against him. Books four and five deal with this critical struggle until 1479. During the second phase of the struggle Uzun Hasan was to become the conqueror's most dangerous rival. Babinger's treatment in books four and five does not make a significant addition to what was previously available in the literature. For the key role played by Uzun Hasan in this period see John Woods's, *The Aqqyunlu: Clan, Confederation, Empire* (1977).

In book six the author outlines the two final campaigns of Mehmed II against Rhodes and Otranto (1480), which were the prelude to his planned invasion of Italy. Book seven is devoted to a discussion of the character and personality of this exceptional man. Babinger was a specialist on Mehmed II's interest in Italy, and he provides us with some fresh insights in this chapter. In the same chapter he also deals with Ottoman state and society under Mehmed II. Even in such a general book, however, one would expect a more substantial treatment of the Ottoman legal system, the army, the navy, finances, and methods of conquest. The author could have made greater use of published archival materials on these topics. The notes prepared by Hickman, however, make up to a certain extent for this lack in the original book. Babinger's book, in sum, despite all its shortcomings, remains the only comprehensive work on Mehmed II's reign.

HALIL INALCIK
University of Chicago

AMOS PERLMUTTER. *Politics and the Military in Israel, 1967–1977*. Totowa, N.J.: Frank Cass. 1978. Pp. xiv, 222. \$15.00.

The author of this book, Amos Perlmutter, has served as a member of the Israeli Defense Ministry's Atomic Energy Commission. The present volume is a sequel to *Military and Politics in Israel, 1948–67* and also elaborates on portions of his recent work, *The Military and Politics in Modern Times*. The author further explores the former book's major thesis: that civilian dominance over the military has persisted and that the Israeli Defense Forces (IDF), as a people's reserve army, remain politically and legally the instrument of the government and the regime in power. Perlmutter sees Israel, however, as still the nation-in-arms that it was in 1948 and as this century's best example of a garrison state.

Perlmutter introduces the book by summarizing the development of Israel's armed forces from their

earliest antecedents—the Hashomer (1909), the Haganah, the Palmach, and other forces—to the modern Israeli army. David Ben-Gurion, the architect of the state of Israel, masterminded the depoliticization of the IDF to fit his concepts of an independent Zionist democratic republic. By the Six Day War of 1967, the first war that Israel undertook without Ben-Gurion at the helm, his doctrine of civil-military relations had become an enduring legacy.

After Ben-Gurion's retirement, Prime Minister Eshkol allowed the IDF to take over the sole conduct of military strategy, but the decision to resort to the use of force remained a cabinet prerogative. In the weeks preceding the Six Day War, the high command had little hope for diplomatic successes or for American intervention on behalf of Israel. By May 18 General Moshe Dayan, then outside the senior political and military councils, was arguing that war with Egypt was unavoidable. On June 2 a coalition government was established in response to pressure from public opinion and the opposition parties. Dayan became defense minister, and the political leadership allied with the IDF in determining to wage war. The world knows of the completeness of the lightning-like victory that followed, but its luster soon faded in the long war of attrition and the Yom Kippur War of 1973. Perlmutter's account of the political and military misperceptions that paved the way for Israel's reverses at the outset of its fourth war is among the more useful parts of the book. A substantial portion of this work deals with crisis management, especially during Henry Kissinger's Middle East negotiations. Perlmutter closes with an account of civil-military relations during the period 1973-77 and a useful epilogue on the Entebbe rescue of 1976.

The author interviewed many key figures, including most of the Israeli generals who have served since 1967. His extensive footnotes cover the pertinent literature for the period, but Perlmutter states that the Israeli government will not give scholars access to documentary material on the IDF "until the wars between the Arabs and the Jews are ended" (p. 4). Until then, this book will help both general readers and scholars understand Israel's political-military relations as well as those with its Arab neighbors and the superpowers. The author makes no claim to have written a definitive history, but his book is a valuable treatment of these important subjects.

ELMER B. SCOVILL
Alexandria, Virginia

GEORGE LENCZOWSKI, editor. *Iran under the Pahlavis*. (Hoover Institution Publication, number 164.)

Stanford: Hoover Institution Press. 1978. Pp. xxii, 550. \$19.95.

These twelve essays by different authors discuss aspects of Iran's development in the twentieth century. Chapters survey the reigns of both Pahlavi Shahs; social, cultural, and educational developments; economic affairs and oil; foreign policy; the army; the tradition of kingship; and literary achievement. The contributors—from North America, the United Kingdom, and Europe—are mainly established authorities.

Iran's unique role in modern world affairs creates a need for books of this type. If one is to understand Iran today, one must understand the Iran of yesterday. Roger Savory's chapter on social development rests on a solid historical base and carefully relates developments in law, public health, and the status of women to the political affairs of the time. Charles Issawi's contribution on the Iranian economy from 1925 to 1975 is useful, as is Alvin Cottrell's work on the armed forces.

There seems to have been little attempt made to coordinate the chapters. Readers will find provoking discrepancies. The author dealing with Iranian foreign policy wrote of Iran's participation in the Dhofari rebellion: "To this the Iranian military presence had contributed significantly—not, perhaps, as much as Tehran liked to think, but more than the Sultan's British officers were willing to admit" (p. 380). This seems hardly consistent with what is said in the next chapter: "The British commander of the sultan's armed forces, Major General K. Perkins, paid them proper credit: 'The war would certainly not have been won as quickly as it was without the Iranian assistance'" (p. 411).

Most of the contributions rely on secondary, frequently outdated, material. There is now available fresh documentary evidence of massive proportions in public archives and in private collections of which hardly any use was made. Some chapters are little more than weary repetitions of old saws (as in the chapter on oil) or at best a lucid restatement of work already printed. Not much is new in either the editor's contributions on political evolution in Iran or in D. R. Denman's chapter on land reform. A glaring omission in the treatment of land reform (discussed in more than one chapter) is the lack of reference to the early work of Minister of Agriculture, Hasan Arsanjani.

The book seems to have been written some time before publication. It mentions Iran's attempts to industrialize while sizable oil revenues are still coming in but has no analysis of priorities allotted, problems encountered, or resources available. On the political side, the reader would not learn that for many Iranians Musaddiq is still a national hero. There is no treatment of the religious teach-

ers as a current political force. Terrorism is also omitted. There is no allusion to present-day difficulties, which are increasing, not dwindling. The account of American-Iranian relations during the last quarter century is roseate. There is no reference to the Shah's resentment of the alleged American insistence on appointing Ali Amini as prime minister in the early 1960s. The alleged American role in events leading to the coup in August 1953 is not examined, although we are directed in a footnote (p. 374) to another chapter where, in fact, the episode is barely mentioned.

There are errors of fact. On page 370 the writer states: "The British had acquired practically complete control of the southern partly-Arabic-speaking [sic] province of Khuzistan, became the protectors of its tribal chieftain, Shaikh Khaz'al, and thus consolidated their hold on Iran's oil production." Shaikh Khaz'al's authority did not extend into northern Khuzistan where the oil fields were situated in Bakhtiari lands. Britain entered into special relations with Shaikh Khaz'al originally because of German penetration into the Gulf. Oil became important later with the building of the Abadan refinery. On pages 12-13 it is not stated that it was on General Ironside's insistence that the Russian officers were dismissed and that it was Ironside's decision, not an Iranian one, to appoint Reza Khan to command the Cossacks. Referring to page 378, the British, not the Shah, took the initiative in settling the Bahrain dispute and in establishing a federation of the Sheikdoms. The Shah at first opposed the idea of federation.

Despite its defects, which are many, the book is worth having. Chapters dealing with social traditions, culture, literature, and education are especially interesting. Besides other illustrations there are three rather overwhelming ones in color.

ROSE LOUISE GREAVES
University of Kansas

AFRICA

LUCETTE VALENSI. *On the Eve of Colonialism: North Africa Before the French Conquest*. Translated by KENNETH J. PERKINS. New York: Africana Publishing Company. 1977. Pp. xxix, 154. Cloth \$15.50, paper \$8.50.

In this work Lucette Valensi does not fulfill the complex purpose stated in her preface but does provide a useful, relatively short introduction to the precolonial history of North Africa. She offers a basic survey of the major social, economic, and political issues in North African life in the period 1790-1830. Even more useful to the uninitiated, she summarizes historiographical disagreements and

cites most of the major studies that have been written about the subject. Her own interpretation—decidedly but rationally anti-imperialist—is also made clear. The book concludes with a sampling of documents that aid in her historiographical review but add little to her less successful attempt at a new interpretation.

Unfortunately, Valensi begins her book with the statement that: "as deep as the trauma of conquest and colonialism may have been, and as destructive as their effects on some segments of North African life were, some old structures did resist, were reformed, and functioned, in spite of changes. These forms, eroded but still discernible, can never be ignored in any model for development. This work attempts to show them in their precolonial pattern" (p. xiv). This acknowledged take-off from the brilliant interpretations of Jacques Berque promises much that is never delivered. Equally unfortunate, Valensi has much more to say about Morocco and Tunisia than about Algeria. This is understandable, but it would be more acceptable if she explained that the timing and violence of French imperialism in Algeria make it difficult to know what occurred there before 1830. One useful approach to the immediate precolonial period in Algerian history is to study the resistance period (1830-47). The Amir Abd al-Qadir and his contemporaries were products of precolonial society, and studying them tells us much about Algeria before 1830, as Raphael Danziger has demonstrated in his *Abd al-Qadir and the Algerians* (1977). Danziger's introductory material provides more professional insight into precolonial society than does Valensi.

Nevertheless, Valensi provides a good introduction to precolonial North African history. Her book might be used profitably in the classroom or by the nonspecialist who wants a quick glimpse at what occurred in and what has been written about the Maghrib in the period 1790-1830.

RICHARD ROUGHTON
University of Rhode Island

FREDERICK COOPER. *Plantation Slavery on the East Coast of Africa*. (Yale Historical Publications, Miscellany, number 113.) New Haven: Yale University Press. 1977. Pp. xviii, 314. \$18.50.

Though long a neglected area of inquiry, the study of African slave systems has developed in recent years almost as rapidly as the study of similar systems in the Americas. The best study yet to appear in English and the first to go into depth about an African plantation system is Frederick

Cooper's outstanding monograph, originally written as a doctoral dissertation at Yale.

Cooper focuses on three communities—Zanzibar, Malindi, and Mombasa. The island of Zanzibar became the world's major source of cloves in the nineteenth century when its plantations were the largest market for the rapidly expanding East African slave trade. Malindi was a virtually deserted city on an underpopulated stretch of the Kenya coast that developed rapidly in the late nineteenth century as a source of grain for Arabia and Zanzibar and a producer of sesame and coconuts for the world market. Mombasa, also on the Kenya coast, was a major terminus for trade routes into the interior, but its plantation development was hindered by greater population density. Less free land was available, units of production remained smaller and less efficient, and trade remained more important than agriculture.

The strength of this study lies in part in Cooper's comparative perspective. He writes perceptively of the way in which both Islam and the culture of the Omani Arabs shaped and legitimated slavery. Plantation systems developed, however, largely because of the demands of an expanding world market and the opportunities presented by fertile soils. This opens up parallels with the Americas. Here Cooper has read widely. He constantly and insightfully uses comparisons with the United States, the West Indies, and Latin America.

Cooper's most important theme seems to be taken from the observation of David Brion Davis that there is a contradiction between the slave's humanity and the legal definition of him as a thing (*The Problem of Slavery in Western Culture* [1966], p. 58). Cooper attributes great importance to the delicate "balance of power" between master and slave. Lacking the coercive potential of his American counterpart, the Arab or Swahili slaveowner had to combine punishments and rewards in different ways and make less harsh demands on the slave. The slave had few cards up his sleeve, but he could escape or resist and this possibility shaped the development of slavery.

In spite of the effects of economic rationalization, precapitalist social relationships persisted in East Africa. Though slaveowners and slaves both constituted well-defined social classes, links between superior and inferior tended to be more important than those within either class; as a result the system tended to rely much more on consent than on coercion. The Zanzibari or Malindi plantation also never achieved the opulence of its American counterpart. This was true partly because the profits from cloves and grain were less than those from sugar and cotton but also because the goals of the ruling class were different. Life was

simpler, consumption less extravagant, and the plantation itself a more self-sufficient unit.

All of this Cooper does with impeccable skill. This monograph is an important addition not only to African history, but also to the study of slavery in comparative perspective.

MARTIN A. KLEIN
University of Toronto

EDWARD I. STEINHART. *Conflict and Collaboration: The Kingdoms of Western Uganda, 1890-1907*. Princeton: Princeton University Press. 1977. Pp. ix, 311. \$18.50.

Edward Steinhart's book provides two important contributions to students of African history. First, the book contains valuable information on the history of three Western Ugandan kingdoms—Ankole, Toro, and Bunyoro—during the early years of British colonial rule. Second, Steinhart's discussion of the nature of resistance and collaboration in these areas should be immensely useful to anyone interested in these themes in the wider context of African colonial history.

Steinhart's basic approach is interpretive and comparative, and he borrows the "controlled comparison" methodology of ethnographers and sociologists. His concern is to compare the responses of three essentially similar polities to the imposition of British colonial rule. Throughout this comparison, he focuses on political elites of the three kingdoms, whom he sees as the most significant agents of resistance and collaboration.

In the introductory chapter, a short synopsis of the origin of each of the kingdoms is provided, together with a discussion of such themes as political recruitment and social stratification. The next six chapters examine the various responses of the three kingdoms to the advent and implementation of British rule. Each kingdom is given thorough and even attention, and their individual reactions are comparatively analyzed. All of these chapters are well done, but the ones examining Nyoro resistance (chap. 3) and the establishment of collaboration in the three kingdoms (chap. 6) are particularly successful.

Perhaps the strongest section of the book is the treatment of collaboration in chapter eight. In many of the pioneering works on African resistance, collaboration has received only cursory attention and has been viewed rather simplistically as a negative response, diametrically opposed to resistance. In Steinhart's words, "some of the complexity and variety of African responses have been hidden in the long shadows cast by the heroes of resistance" (p. vii). Through his careful historical reconstruction, he persuasively demonstrates that

in fact collaboration can be seen as a "fluid and creative response . . . to a situation of inherent conflict and compromise" (p. 257) and thus gives us valuable insight into that previously hidden complexity and variety. While earlier works by William Ochieng' and the Isaacmans, among others, have contributed significantly to the study of collaboration, Steinhart's analysis is the most perceptive I have had the pleasure of reading.

The book has two relatively minor weaknesses, neither of which detracts significantly from its overall success. Although the author apparently appreciates the importance of oral materials, and indeed conducted an impressive 141 oral interviews with informants from all three kingdoms, he does not often directly refer to these materials. Beyond this, more information on interviewing methods and selection of informants would have been appropriate. The second criticism concerns the focus on the recruitment and tenure of political elites, somewhat to the exclusion of other factors, notably external ones such as the question of British colonial policy, which are treated as "constants." Although this primary focus on the elites seems justified, one wonders whether sufficient attention has been paid to other important considerations, such as the role of the peasantry.

Nevertheless, Steinhart's achievement is significant. In the final paragraph of the last chapter (p. 274) he says that if his comparative methodology, with its focus on the chiefly elites, "provides new insights or a new dimension to our understanding of the problems illustrated by the histories of these kingdoms and does not obscure other possible views and analyses, it will have succeeded in achieving its purpose." Steinhart should rest assured that it has succeeded and admirably so.

JOHN LAMPHEAR
DePaul University

ROBERT I. ROTBERG. *Black Heart: Gore-Browne and the Politics of Multiracial Zambia*. (Perspectives on Southern Africa, number 20.) Berkeley and Los Angeles: University of California Press, 1977. Pp. xviii, 359. \$15.00.

Robert I. Rotberg, the distinguished Africanist and Haitianist, has written a fascinating biography of Sir Stewart Gore-Browne, probably the most important political figure in Northern Rhodesia in the late 1930s and 1940s. Rotberg's most significant accomplishment is to reveal the evolution of a mind and character molded at the end of the Victorian era in a talented upper-class family, at Harrow, and in the artillery. Sent to Northern Rhodesia, Gore-Browne found his Shangri-la, which World War I, however, quickly turned into

a mirage. On the Western front he proved himself an officer of great ability. Despite the carnage on French fields of blood, he remembered his African idyll and returned in 1920 to his beloved Lake of the Royal Crocodiles where he cultivated orange blossoms and geraniums, distilling their precious oils for perfumes although he knew nothing about such botanic exoticism.

Together with his nineteen-year-old bride, he slowly created an English manor with an Italianate mansion, a school, a dispensary, orchards, a cattle herd, and responsibility for innumerable African retainers and tenants. His wife drifted away as the estate became prosperous, and he found solace in colonial politics where he championed the welfare of Africans.

For over fifteen years, after 1935, he befriended and counseled colonial governors, served on commissions, and acted, virtually alone, as representative of African interests. He also served as wartime administrator, lobbyist, and publicist in England for Northern Rhodesian causes. Caught between the settlers' postwar demands for domination and the new African intelligentsia's determination to lead, he retired from the legislature in the early 1950s. Disillusioned from the outset with the Central African Federation, Gore-Browne turned African nationalist, accepted the leadership of Kenneth Kaunda, and used his considerable influence in London to promote African domination. He lived long enough to be hailed in his eighties as one of Zambia's honored founders.

Rotberg analyzes how birth, breeding, education, and military service, together with the sustaining love of a talented aunt, acceptance of the best conventions of his class and age, relative financial security, and the selection of an isolated estate in Northern Rhodesia shielded Gore-Browne from the racism of most white settlers and enabled him to learn about Africa gradually from experience. He was that rare human being who continued his education throughout life while preserving his youthful ideals, humanity, zest, and innate sense of decency.

Black Heart permits the reader to disregard too easily the fact that the economic development of the copper and railway belts was more central to the history of Northern Rhodesia than was political growth. Was this former artillery officer therefore Northern Rhodesia's pre-eminent figure in his heyday? And although Rotberg sympathetically narrates Gore-Browne's alienation of both European and African political trust as he stumbled between two increasingly irreconcilable movements, he does not starkly confront the fact that Gore-Browne's legislative career ended in failure. His last chapters fail to make clear that Gore-Browne's influence never recovered from this de-

bacle and that he had only a peripheral impact upon the dismantling of the Federation and the emergence of an independent Zambia.

Despite its intensive research, the book lacks a comprehensive and exact bibliography of *primary* sources. The footnotes are, however, precise. Rotberg's successful twining of African and British imperial history in this *tour de force* demonstrates that cooperation between these two fields enriches both. This biography proves the importance of the Gore-Browne papers for Zambian history; it is to be hoped that Rotberg will soon edit and publish them.

HENRY P. PORTER, JR.
Washington and Lee University

ROBERT BLAKE. *A History of Rhodesia*. New York: Alfred A. Knopf. 1977. Pp. xxii, 435. \$15.00.

The dust jacket describes this work as a "full and authoritative history . . . written with [Lord Blake's] special understanding of British imperial history [and based on] a wealth of private papers to which only he has ever been granted access." The author declares that "Rhodesian history is a strange and intriguing compound of romance, idealism, courage, arrogance, avarice, and accident." Yet the reader will be disappointed, for this volume meets neither the dust jacket description nor the high standards set by Lord Blake in his biography *Disraeli* (1966).

Although written in a pleasant and engaging style, the volume lacks originality, relies largely on published sources, and shows little acquaintance with serious Africanist and imperial scholarship or analysis of race relations. Only two of the twenty-nine chapters are devoted to the precolonial period and none deals with modern African affairs in their own right, though the author generally concedes that virtually all European activity is conditioned by racial considerations. Lord Blake ignores the Africanist methodological and historiographical advances of D. Beach and J. Cobbing and the imperial studies of J. S. Galbraith as well as the older pioneering work in race relations by R. Gray.

If the book has particular value it lies in Blake's account of the Westminster model of parliamentary government in Rhodesia, and more particularly, of the role of the prime minister (Lord Blake's own specialty). He deals at length with Godfrey Huggins, Garfield Todd, Sir Edgar Whitehead, Winston Field, and Ian D. Smith, and with the exception of Smith, he had access to private papers of all these men. Nonetheless the information and interpretation he gives of Huggins's twenty-year prime-ministership does not

supersede the biography by L. H. Gann and M. Gelfand (1964). The Todd and Field private papers, moreover, reveal nothing of the men or the political machinations that has not already appeared in studies on the collapse of the Federation.

Although ostensibly critical of white rule, Lord Blake indirectly reveals admiration for Rhodes's higher aspirations and the economic achievements of the white settlers. He seems to defend the migratory labor system and the attendant compounds as preferable to permanent urban ghettos. Similarly he accepts government statistics on the improvement of African wages during the 1950s. Neither of these claims to paternal benevolence will bear scrutiny.

It is unfortunate that Lord Blake did not take full advantage of the recent journal and monographic literature on Rhodesia/Zimbabwe. His graceful prose will not stand as the "full and authoritative history" of this troubled land. Such a volume has yet to appear and will have to be more sensitive than this one to the African majority and to economic issues.

OLIVER B. POLLAK
University of Nebraska

GERALD J. BENDER. *Angola under the Portuguese: The Myth and the Reality*. (Perspectives on Southern Africa, number 23.) Berkeley and Los Angeles: University of California Press. 1978. Pp. xxviii, 287. \$15.00.

A newcomer to the study of Portugal in Africa, fresh from a visit to Mozambique where it was obvious that Africans had endured excesses of racist exploitation not practiced by other colonial powers, I was surprised that this scholarly denunciation of Portuguese policies in Angola had to be so vigorous. But in picking a volume at random, Robin Hallett's useful *Africa since 1875* (1974), I read that "the Portuguese colonies never gave birth to so rigid a racist philosophy as that developed in other settler communities in southern Africa" (p. 512). And if the Portuguese receive faint praise in many non-Portuguese works, Portuguese writers often interpret their colonial past in an entirely different manner, claiming special qualities for their rule, especially its supposed multiracial virtues. "It has been difficult," observes Gerald J. Bender, "for many non-Portuguese to understand how the overwhelming majority of the Portuguese people could sincerely believe that their perspective was realistic" (p. 204). With the publication of this seminal monograph the problem no longer remains.

Bender surveys the Portuguese overseas record

from the end of the fifteenth century until the mid-1970s, subjecting their various beliefs to penetrating analysis. The doctrine of Portuguese suitability for living with other races, so eloquently expressed by Gilberto Freyre, is shown, through examination of race relations in Africa and Brazil, to be a sham. Bender argues that the few Portuguese who settled in Angola over the centuries—mostly criminals exiled from their homeland—demonstrated the worst possible qualities for Europeans residing in colonial territories. The constant denigration of these immigrants—Portuguese “scum” (p. 62) to Bender—is perhaps justified, but to me it indicates a loss of the scholarly detachment that usually characterizes this study. Criminals were not exported to South Africa: were Africans any less mistreated there? And when Bender speaks of the criminals as undermining respect for Portuguese civilization, when elsewhere he clearly demonstrates that most Africans in this vast land had minimal contacts with the colonizers, is he not unnecessarily falling into the mental framework provided by the Portuguese?

These minor cavils aside, Bender demonstrates that in Angola government policies for planned agricultural settlement, despite repeated efforts, led to little result—by 1950 less than 10 percent of the whites in the colony worked in agricultural endeavors. When the war for African independence began in 1961, the Portuguese partially recognized the futility of past policies, yet in the end they proved unable to modify their conduct enough to stem African resistance.

Taken as a whole, Bender's lucid portrayal of Portugal in Angola is damning to the enduring myth of their relations with Africans. His conclusions, backed by ample evidence, should influence all future studies of overseas Portugal. Hopefully Bender's work will be translated quickly into the Portuguese language so that its message may enlighten Portuguese perspectives in present and future encounters with their former colonial subjects.

NORMAN ROBERT BENNETT
Boston University

MARYNA FRASER and ALAN JEEVES, editors. *All that Glittered: Selected Correspondence of Lionel Phillips, 1890-1924*. Cape Town: Oxford University Press. 1977. Pp. 428. \$31.00.

Lionel Phillips was a central figure in the development of gold mining in South Africa, and his career, as mirrored through his correspondence, reveals a great deal about the industry's impact on the country during his active years in business.

During the course of his thirty-five years in the industry, Phillips served the three great mining houses of H. Eckstein and Company, Wernher, Beit and Company, and Central Mining and Investment Corporation Limited, which were known collectively as the Corner House group. His correspondence throughout this period is now housed in the Barlow Rand Archives in Johannesburg, and the 172 letters selected for this work are illustrative of the significant interaction between business and politics in South Africa. The editors, both of whom have ideal credentials for an undertaking of this nature (Maryna Fraser is archivist of Barlow Rand and Alan Jeeves is a historian at Queen's University, Canada, who has specialized in the formative stages of South Africa's mining industry), have chosen their material well and provide all the scholarly appurtenances so vital to a work of this nature.

The introduction is a solid one, but it would have been further strengthened by the inclusion of biographical information on the personality and character of Phillips. One develops a strong empathy for Phillips as a forthright, efficient businessman, but the nature of the letters leaves the reader with an unsatisfied curiosity regarding other facets of his life. On the other hand, the appendixes, which include biographical notes on individuals mentioned in the letters, information on mining companies, and a glossary of mining terms, are uniformly well done and helpful. The same is true of the index and select (but extensive) bibliography.

Phillips's correspondence seems destined to become something of a classic of its genre. Phillips was a man of keen intelligence, and his letters range across a broad spectrum of issues confronting the mining industry as well as South Africa generally. They offer a shrewd assessment of the effects of both the Boer War and the First World War on business enterprise, provide suggestive and frank comments on prominent politicians and entrepreneurs, and delineate the reactions of one farsighted individual to changing forces within the society in which he lived. Phillips was a fine letter writer and the selections printed here display a vitality seldom encountered in correspondence of this type.

This welcome addition to the available source material on South African mining history provides much food for thought on the broader economic and political issues facing the country during Phillips's career. One hopes that the appearance of this work will stimulate one of the editors or some other scholar to undertake a full-scale portrait of a fascinating and important figure.

JAMES A. CASADA
Winthrop College

HARRISON M. WRIGHT. *The Burden of the Present: Liberal-Radical Controversy Over Southern African History*. Cape Town: David Philip. 1977. Pp. vi, 137. R 3.90.

There are no South Africans, runs the title of a South African book published after the end of World War II. South Africans are divided; they lack an all-embracing sense of nationhood. The same applies to historians of Southern Africa. No common method, approach, or even subject matter unites their work; each account tends to embody the standpoint of a particular social group or of a particular ethnic community. George McCall Theal, the forgotten giant of South African historiography, idealized the white frontiersman. The Theal tradition still echoes faintly in the writings of Peter Duignan and myself; both of us are apt to admire the entrepreneur, the farmer, the prospector, or the factory owner—regardless of color. Afrikaner historians like P. J. van der Merwe, on the other hand, have emphasized the Afrikaners' role in South African history, especially the part played by the *trekboer*. Liberal historians of pre-World War II vintage used to give short shrift to the Afrikaners, and instead praised the British liberal imperial tradition and the work of missionaries. The modern liberals, writers like Leonard Thompson and Monica Wilson, wish to lay more emphasis on the black share in South African history; they deplore what they regard as South Africa's racist past and look to a future in which all races will peacefully share the land. The liberals (including T. O. Ranger, a historian not mentioned in the work under review) have been challenged in turn by radicals, such as Giovanni Arrighi and Martin Legassick, who accuse all their predecessors of distorting history and who place their trust in the revolutionary transformation of South African society.

Harrison M. Wright's thoughtful and incisive critique of this amorphous tradition makes a valuable contribution to South African historiography. He finds that liberals and radicals have much in common—an underlying optimism regarding the future of humanity, a commitment to what they consider a more just form of society. But they also share similar failings: "a tyranny of preconceptions or of theory, an excessive eye on the present, a literal projection of one's own thinking into the minds of others" (p. 95). Radicals, as Wright points out, have made a contribution to scholarship in their re-evaluation of the old-style liberals' *bête noire*, the Afrikaner frontiersman—here and in some other respects their criticism runs oddly parallel with the views of old-style conservatives. But radicals, as Wright shows, have also been guilty at times of shoddy scholarship, skimpy research, and an unfortunate addiction to

ad hominem arguments. In terms of scholarly output, one might add, their achievement is miniscule compared to those of men like Theal, a self-taught historian who published more than fifty volumes of historical works and archival documents.

Wright might also have criticized radical and liberal scholars alike for their frequent inability to understand their own preconceptions within the wider framework of society. Intellectuals, liberal as well as radical, are often influenced in their critique of white South Africa by their widespread suspicion of, or disdain for, "Middle America" or its British equivalent. The intellectuals' esthetic, moralistic, or class-bound prejudices against "hard hats" or bourgeois philistines in their own society are all too easily projected on the Southern African screen.

Radicals, although claiming to be Marxists, are also unwitting revisionists of the master's doctrine. Marx, in the *Communist Manifesto*, praised the historic achievements of the bourgeoisie in terms that paralleled Macaulay's; and his evaluation of the British role in India scarcely differed from Kipling's. Marx assumed that the bourgeoisie would cease to be progressive when bourgeois rule began to shackle economic progress; but he never denied that the bourgeoisie had played a progressive role. Modern radicals, on the other hand, have evolved their own doctrine of "African exceptionalism." They do not concede to the white bourgeoisie, either South African or metropolitan, a progressive role at any stage in the history of the continent. Radicals forever pit perpetually wicked white capitalists against eternally virtuous black peasants and proletarians. History then becomes a global melodrama that Marx would surely have treated with contempt.

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MICHAEL SILAGI. *Von Deutsch-Südwest zu Namibia: Wesen und Wandlungen des völkerrechtlichen Mandats*. (Abhandlungen zur Rechtswissenschaftlichen Grundlagenforschung, number 29.) Ebelsbach: Verlag Rolf Gremer. 1977. Pp. ix, 165. DM 42.

The main title of this extended essay may mislead potential readers. This is not a historical study of Southwest Africa/Namibia or even of the development of its position in international law. Michael Silagi rather presents a meticulously documented, cogently argued critical commentary on the opinions of the International Court of Justice in the five Southwest Africa cases (1950, 1955, 1956, 1962–66, 1971) in which the court, by stages, first qualified

and eventually denied the legal basis of South African sovereignty over the territory.

Basing himself solidly on the widest range of relevant historical as well as legal literature, Silagi argues convincingly that sovereignty over the former German colony was conferred upon South Africa by the principal Allied and Associated Powers in 1919; that the Mandate Agreement of December 1920 did not detract from that sovereignty and provided for no sanctions against default of the obligations undertaken by South Africa, minimal as these were; that the U.N. claim to have succeeded to the legal position of the League with regard to Southwest Africa was legally unsound; and that thus no legal basis existed for the U.N. actions of 1966 and 1970 by which it terminated the mandate and declared South Africa's presence in the territory illegal. The Court validated these actions in 1971. Silagi feels that the case demonstrates the misuse of international law to achieve ends that should have been sought through political and moral means.

Perhaps. Any historian will agree that the inventors of the mandate system, especially Jan Smuts and Woodrow Wilson, who formulated it at a time when colonial rule—and racism—were matters of course, would have been astonished, and probably appalled, by the meaning read into the Southwest African mandate agreement two generations later. The Class C mandates, Southwest Africa among them, were clearly intended as a minimal fig leaf for annexation by the adjoining dominions. Historians, however, also know that the interpretation of law, domestic or international, is never unaffected by changing political realities and moral climate.

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ASIA AND THE EAST

R. W. L. GUISSO. *Wu Tse-t'ien and the Politics of Legitimation in T'ang China*. Preface by E. G. PULLEYBLANK. (Occasional Papers, Program in East Asian Studies, Western Washington University, number 11.) Bellingham: Western Washington University. 1978. Pp. xii, 335. \$16.00.

Wu Tse-t'ien was empress of China for fifty years. She supplanted Kao-tsung's first empress in 656, and, by 664, she had become an active partner on the throne, sharing court decisions and initiatives with the emperor. After he died in 683 she gained full authority, quickly deposing her son Chung-tsung and replacing him with her more compliant son Jui-tsung. In 690 she declared herself ruler in her own right, a female emperor not an emperor's

wife, and changed the name of the dynasty from T'ang to Chou. In 705, as she approached her eightieth birthday, she was forcefully retired and Chung-tsung restored to the throne. Famous for her cruelty, intrigue, and weakness for favorites, Empress Wu has not fared well in popular historical memory. This new monograph by R. W. L. Guisso should correct many of the injustices done to her.

As the title suggests, how Empress Wu legitimated her rule is a major theme of the book. Guisso discusses her use of Confucian, Buddhist, and Taoist ideology, but concludes that what in the end led people to accept a female ruler was her competence. Unfortunately, his sources do not allow him to demonstrate that people actually viewed her as competent, but he can show that few took part in the attempts to overthrow her and that she was indeed politically astute.

Guisso's main contribution, however, is his assessment of Empress Wu's place in Chinese history. He fully examines the sources that relate to her motives, methods, and achievements. Her greatest accomplishment in his eyes was her success in enhancing the power of the throne; she reduced the independence of aristocrats by expanding the examination system, and she weakened the power of the chief ministers through increases in their numbers and frequent transfers. Yet assigning her credit for these changes raises many questions of interpretation. How much did Empress Wu contribute to measures undertaken while Kao-tsung was alive, especially during the early years of his reign? How much of the "bureaucratization" of the aristocrats took place in earlier reigns, even earlier dynasties, especially the Northern Wei, Liang, and Sui? Indeed, was enhancing the power of the throne at the expense of the chief ministers something she should be praised for? Guisso has opened the debate on these issues by presenting a brief on behalf of Empress Wu, giving her the benefit of every doubt, but other scholars will surely dispute many of his points.

One reason why Empress Wu has fared so badly at the hands of historians is her usurpation of the throne. Guisso takes as generous a view of this as he can, finding justifications for her violent purges and pointing to the difficulties a woman faced in ruling. He proposes that she was never disloyal to the T'ang but, on the contrary, fully expected the restoration of the dynasty on her death. He offers no satisfactory explanation for why she changed the name of the dynasty but does note that she did not appoint a Wu nephew as heir, but one of her own sons, simply having him change his name from Li to Wu. Guisso alludes to but does not fully explore the ambivalence Empress Wu apparently

felt as a female ruler in a patrilineal society: Was she a Wu? Was she a Li? Whom did she want to succeed her, a son, a nephew, a daughter?

In sum, this book deals with many of the major questions of T'ang history, indeed of traditional Chinese history, and will be read with profit by scholars and advanced students. Its presentation is lucid and careful, fully documented with maps, notes, and lengthy quotations from original sources that allow the reader to follow the arguments closely.

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DAVID D. BUCK. *Urban Change in China: Politics and Development in Tsinan, Shantung, 1890-1949*. Madison: University of Wisconsin Press. 1978. Pp. xvi, 296. \$15.00.

In this fine study of Tsinan, the capital of the important province of Shantung, David D. Buck has set out to analyze the inland, non-treaty port city more in the tradition of Chinese "geography-history" scholarship than in that of the social science approaches of scholars of urban China. Recognizing and benefiting from the contributions of such urbanists as G. William Skinner, Gilbert Rozman, and others, Buck has chosen to place his study in a chronological framework, relating Tsinan spatially to Shantung as a whole and temporally to changing contemporary circumstances. By this method he seeks to answer two questions: what historical factors explain the growth and stagnation of this city, and how this experience relates to the role of cities in modern Chinese history. In my opinion he has succeeded quite well in answering the first question, but only partially the second.

The organization of the book reveals Buck's research design. He discusses Tsinan serially through the late Ch'ing, early republic, warlord, Kuomintang, wartime, and postwar periods. Buck evaluates the impact of three factors on Tsinan: foreign influence, economic growth, and political leadership. He shows persuasively how political leadership, from the time of Governor Yuan Shih-k'ai (1890s) to the rule of Han Fu-ch'ü (1930s), has always been the dominant factor in Tsinan's growth and development. While the local gentry did wield considerable political and economic power, their interests were ultimately subordinated to extra-provincial considerations. As a result Tsinan did not develop as rapidly as it might have up to 1925, when it suffered under the brutal warlord Chang Tsung-ch'ang, and thereafter did not progress until after 1949. By attributing this

failure primarily to vestigial warlordism and secondarily to the widening developmental gap between Tsinan and its rural hinterland, Buck takes issue with the traditional explanation, which assigns blame primarily to foreign imperialism and general economic decline.

The study highlights several important aspects of Tsinan's history: the traditional rivalry between coastal and interior Shantung, a historical split that traced back to the warring states of Ch'i and Lu in Confucius's times; the influential but by no means dominating factor of treaty port competition, exemplified in this case by Tsingtao at the coastal end of the German-built Tsingtao-Tsinan railway; and the interplay of the top provincial leader with the local gentry community. Buck's brief portrait of the independent governor Han Fu-ch'ü is especially effective. The Japanese-inspired Tsinan Incident of 1928 is also judiciously handled. Yet Buck's research design has certain intrinsic problems. Since the political history of Tsinan was inseparable from that of Shantung, a fact he well recognizes, he has had to conduct the discussion of politics beyond the level of the city itself. Therefore large portions of the account (especially chapters four and five) treat Shantung as a whole and refer to Tsinan only peripherally. Despite Buck's efforts to integrate the discussions at the provincial level with the largely descriptive account of Tsinan's economy at the local level, he has not entirely succeeded. Another caveat is his treatment of Tsinan since 1949. Possibly because he, more fortunate than most other China scholars, visited the locale of his scholarly interest during a trip to the People's Republic, he has included that period in the account. But the section is so brief and impressionistic that it remains quite gratuitous.

In sum, Buck has given us the first systematic study of this important city. Drawing upon Chinese, Japanese, and Western sources available to him, especially British and American consular reports, he has provided us with a wealth of useful information, including data on Tsinan's industry and population found in the appendixes. As urban history this book will satisfy the historian more than the social scientist. It is to be hoped that it will encourage the study of other key inland cities, such as Wuhan, Loyang, and Chungking, giving us thereby a firmer grasp on the precise influence of cities upon the development of modern China.

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FRANZ MICHAEL. *Mao and the Perpetual Revolution*. Edited by I. E. CADENHEAD, JR. Woodbury, N.Y.: Barron's. 1977. Pp. xvii, 326. \$9.95.

The death of major political leaders provides an opportune time for biographers to satisfy readers who are impatient for historical verdicts. Paradoxically, such occurrences are rarely accompanied by the appearance of new historical material and constitute only the beginning of events and trends that may ultimately place a person's life into meaningful historical perspective. Thus, while Franz Michael's *Mao and the Perpetual Revolution* is in a sense timely, the book sheds no new light on the man who dominated China's political arena for three decades and who had been at the forefront of revolutionary change for three decades before that.

The volume covers Mao's life and the events that surrounded it in slightly more than two hundred pages and is clearly intended for the general reader. In this rapid survey of the important features of twentieth-century Chinese history, the participants, including Mao, unfortunately remain two-dimensional. For the most part, they are depicted as single-minded adherents to extremely broad ideological positions, such as institutionalization (in the case of Mao's opponents) or perpetual upheaval (in Mao's case). Above all else, in Michael's scenario, Mao and his opponents were engaged in a struggle for power. While such struggles cannot be ignored, especially in the context of the problem of succession to Mao that dominated Chinese politics in the 1970s, China's leaders also had to deal on a daily basis with a myriad of policy issues related to the concrete problems of managing a large and complex society undergoing rapid change. These issues and problems appear almost incidental in the politics described by Michael. Many of Mao's own writings during the collectivization and communization movements of the 1950s show his pragmatic and flexible approach to policies in the process of experimentation and evolution and reveal that he was capable even of admitting his mistakes. In contrast, Michael's portrayal of Mao as a ruthless and self-serving leader fanatically attached to radical goals is enormously oversimplified. In his effort to emphasize certain traits of Mao's character as persistent, Michael has regrettably ignored the dynamic side of a political leader whose moods, concerns, and analysis of his environment were constantly changing.

Even if a book is directed to the general reader, it should provide some reliable guidance to established works on Mao as an intellectual and political figure. Yet the bibliography glaringly omits such important studies as Meisner's and Wakeman's, or even Pye's controversial but challenging psychobiography, and includes, instead, such items as a *Life* magazine article. The book evidently went to press late enough in 1977 that, for

readers interested in Marxism, some reference might have been made to the symposium on Mao and Marx carried in *Modern China* in 1976-77. Also, although fifty pages of excerpts by Mao are appended to the book, for the reader interested in more primary material fuller reference should have been made to the various editions and collections of the very revealing unofficial works of Mao obtained outside China between 1969 and 1973.

The author's hope was that this book would "stimulate debate with the specialists." Although his interpretations are clearly open to disagreement by other scholars, Michael's conclusions are neither sufficiently focused nor argued systematically enough for a fruitful debate to ensue.

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LYNN T. WHITE III. *Careers in Shanghai: The Social Guidance of Personal Energies in a Developing Chinese City, 1949-1966*. Berkeley and Los Angeles: University of California Press; for Center for Chinese Studies. 1978. Pp. xiv, 249. \$13.50.

In the opinion of its new Communist administrators in 1949, Shanghai was simply too populous. Too many people lived there, and more wanted to move in. Furthermore, the administrators subsequently discovered the unhappy truth that it is easier to train a skilled elite than to employ it, especially when people insist on remaining in Shanghai, China's largest city and one of the three or four largest urban concentrations in the world.

Relying mainly on local newspapers supplemented by interviews with former residents of the Peoples' Republic now living in Hong Kong, Lynn T. White III traces the various remedies used to treat the problem from 1949 through the mid-1960s: workers were temporarily or permanently reassigned to locations outside the city, unemployed peasant migrants were escorted back to their villages, and recent graduates were inspired to go to the countryside to learn class consciousness and to help the country. The party occasionally cajoled or coerced them into taking these assignments. Those who remained were kept busy in new schools (of varying quality) and at makeshift jobs. Meanwhile, the planners campaigned to lower the birthrate and attempted to improve the quality of housing. They did not, however, want to construct too many new apartment buildings, for fear that this additional space would only encourage new migrants and result in an equally crowded and more populous Shanghai.

The administrators also resorted to more direct and sometimes repressive measures. One needed a travel permit to enter the city, but none to leave. All household members had to be registered at the local security station. Even the presence of temporary visitors had to be reported. Certified residents of Shanghai were issued ration tickets; unauthorized individuals were not. In fact, these ration tickets were the principal means of population control, for they guaranteed the holder a set amount of food and cloth at an artificially low price. And since supplies were limited throughout most of this period, and additional goods were hard to find at any price, households encountered many difficulties when they tried to feed and clothe unauthorized members.

Although unpopular, these restrictions did work most of the time. But they did not work when shortages were severe, as in 1955 and in 1961-62, and they were gradually losing their power to limit the population in the mid-1960s, when the supply of goods was becoming both steady and ample. By paying somewhat higher prices for quantities above those guaranteed by the ration ticket, families could provide for unauthorized guests and for children who had left their jobs without permission and returned home. Thus, White suggests that the Cultural Revolution came at a most opportune time in 1966. Not only was the rationing system losing its efficacy, but an awkward surplus of the young and educated was accumulating. While their dedication to the national goals was considerable, it was not always strong enough to overcome their misgivings about departing the urban womb. Some radical new solution was needed.

White set out to do something different than the usual elite study. He wished to avoid the question of "who rules?" and to study the masses in the tradition of Western opinion polls and voting surveys. But from the results offered here it is clear that the goal was beyond his reach because of the available sources. The masses enter this saga only to cooperate with or to resist policies. In fact, the book's central topic is policies, not the policy makers or the masses. Moreover, because the subject is an inhuman one, the book, at times, seems devoid of life and reads like a bureaucratic report.

The most disquieting aspect of this study, however, is that White says he is doing one thing and actually does another. The title promises an account of careers, and throughout the text the author introduces and summarizes the material as if he were analyzing career-shaping policies. But, in truth, he is not dealing with any question other than that of whether or not a career was pursued in Shanghai. What he does provide is a valuable, carefully done study of local population policies that determined how many and what sorts of

people lived and worked in that city. This question, in and of itself, is a worthy topic, and it is unfortunate that the author's failure to recognize it keeps the reader in a state of confusion.

White did succeed, however, in another of his goals: to deemphasize the fact that Communists run Shanghai and to view the city in the more general context of the urban dilemmas produced by a developing economy. That Shanghai is Communist, occasionally, is clear, both because its leaders unabashedly instituted government controls over families and individuals that would be unpalatable in the West in peacetime, but also because its leaders assumed so much responsibility for the unhoused, unschooled, and unemployed. Furthermore, the citizens' undisguised foot-dragging, noncompliance, and ingenuity in going around the regulations reassure one that they do not live in constant fear of their government. One is left with the impression that, although the residents of this great metropolis and their problems are unique in some respects, Shanghai truly is just one more city trying to solve the massive problems that confront urban administrators everywhere, especially in the rapidly changing countries of the Third World.

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J. D. ARMSTRONG. *Revolutionary Diplomacy: Chinese Foreign Policy and the United Front Doctrine*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 251. \$15.75.

One of the most thoughtful and provocative analyses of contemporary Chinese foreign relations to be published in many years, this book by J. D. Armstrong is primarily an attempt to identify the role of ideology in Peking's foreign policy. A secondary concern is to explore changes in Chinese policy, particularly those that might indicate Peking's adaptation to influences from the international system.

In order to achieve these objectives, Armstrong lays out an orderly and systematic plan of attack. First, he creates two alternative models of Chinese foreign policy motivation: (1) a *united front model*, which is drawn from an analogy with the domestic revolutionary experience of the Chinese Communist Party in its own struggle for power, and which assumes that Maoist ideology is the predominant factor influencing Peking's foreign policy; and (2) an *alliance model*, which interprets foreign policy as a calculated reaction to outside threats to Chinese national security, and which assumes that Peking's behavior is determined mainly by China's

geopolitical role in the global system rather than by Maoist ideology. After carefully specifying the two different models in terms of propositions suggesting the kind of actual behavior that would be expected for each, Armstrong then describes the general line of Chinese foreign policy from the founding of the People's Republic in 1949 to 1975, the year before Mao Tse-tung's death. Finally, the author presents four detailed case studies of Peking's relations with those noncommunist countries with which China has had the closest ties: Indonesia, Pakistan, Cambodia, and Tanzania. These are illustrations and, in a sense, tests of the two models.

Armstrong's compelling study deserves a more thorough and extended review than space limitations permit here. In short, it is a pleasure to find a book (which was originally Armstrong's doctoral thesis at Australian National University) so ambitious in scope, so confident and forceful in presentation, and generally so successful in achieving what the author intended. Of particular note, in my opinion, are the systematic and comprehensive character of the study's design and implementation, the clarity and analytical rigor of Armstrong's exposition, and the exceptional quality of his analysis of Chinese policy statements.

The main weakness of the study, in my opinion, is the analogy that Armstrong makes between the CCP's revolutionary experience in China and Peking's role as a state in the international system. Armstrong assumes that such a sharply drawn analogy underlies Chinese united front policy in foreign affairs, but he never stops to examine the fallacies of that analogy (for example, the fact that there is no common culture or integrated social structure among nation states, or that the leadership of a party struggling for power within a country and the leadership of a state making foreign policy cannot be analogous unless you assume that the state leadership seeks to establish direct world domination). Having incorporated elements of this analogy into his united front model, Armstrong may, I think, have created a false dichotomy (united front versus alliance models) when constructing his interpretation of the world view and motivation of the Chinese foreign policy leadership. Despite the fact that Mao Tse-tung used a similar conceptual approach when analyzing domestic politics and international relations, it seems to me that both Chinese theoretical statements and Peking's foreign policy actions demonstrate that the CCP leadership was generally very much aware of the many important ways in which international relations are not analogous to domestic revolutionary struggles.

Nonetheless, there is no doubt that Armstrong's two models provide provocative ideal types that

help us to think constructively about central issues in analyzing Chinese foreign policy. In sum, his book sets a high standard for both conceptual clarity and empirical richness that all of us working on Chinese foreign relations will be hard pressed to equal.

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KENNETH STRONG. *Ox against the Storm: A Biography of Tanaka Shozo, Japan's Conservationist Pioneer*. Vancouver: University of British Columbia Press. 1977. Pp. xviii, 231. \$15.00.

If history, as Edward Hallett Carr wrote, is a continuing dialogue between past and present, then Kenneth Strong has written history. Publication of his biography of Tanaka Shozo may be seen as a consequence of heightened appreciation of man's environment and for those who struggled to preserve it. One hopes not. For Kenneth Strong has written a moving life story of a giant of Meiji times. The son of a village headman, Tanaka Shozo rode the crest of the Popular Rights Movement, first into the Tochigi Prefectural Assembly, which he chaired, and then into the National Diet. He exhibited most of the characteristics of those Meiji men who profoundly shaped the course of Japan's modernization: enormous energy harnessed to a powerful sense of service to his country and commitment to his cause. He was without fear. But he was different from the great men of Meiji. His cause was not the shaping of Japan into a powerful state equal to the great powers. His cause was the little people who suffered as a consequence of this transformation: specifically the peasants of the Shimotsuke Plain, watered by the Watarase River and heavily polluted by the giant Ashio copper works near its headwaters.

Tanaka's battles with government offer a classic case study of the costs of modernization. The inexorable drive to industrialize reached a comic level when the village of Yanaka was declared not to exist in order that engineering decisions could be carried out—comic, were it not so tragic. But Strong does not let the reader relax behind a determinist explanation. At every stage of Tanaka's struggle the author shows that human beings made decisions that had consequences to which there were alternatives. Yet he is careful, too, to remind the reader of the climate of the times, in which Tanaka appeared as a monomaniac bordering on lunacy. Through it all emerges a profound irony. Tanaka himself, in his magnificent effort to correct what he believed—and was—an intolerable situation, was but a microcosm of his society. In his single-minded defense of those who suffered

from pollution, he treated his wife Katsu execrably.

All this Strong describes in elegant prose, on the basis of solid research in primary sources and perceptions gleaned from a thorough knowledge of Japanese society. Tanaka is a perfect example of Ivan Morris's "failed hero." What preserves Tanaka in the memory of the Japanese is not so much the cause as the sincerity with which he pursued it. That he failed in virtually all he attempted is overshadowed by the nobility of his effort. His life then serves as a model affecting those future generations who come to know it. In this sense, history is a continuing dialogue between the past and a gradually unfolding future. Strong has written superb history and thus made us heir to that model.

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MARTINA DEUCHLER. *Confucian Gentlemen and Barbarian Envoys: The Opening of Korea, 1875-1885*. Seattle: University of Washington Press; for Royal Asiatic Society, Korea Branch. 1977. Pp. xiv, 310. \$20.00.

The opening of China, Japan, and Korea in the nineteenth century has been a source of inspiration for historical writing, both academic and popular. Compared with the opening of China and Japan, however, that of Korea has received little serious attention from Western historians. Martina Deuchler's book helps fill this lacuna.

The story of the main diplomatic and domestic events that took place in Korea during the decade from 1875 to 1885 is a familiar one. By concluding the Treaty of Kanghwa (1876), Japan succeeded in opening the hermit kingdom, a tributary state of China, to the world. Appraised of conditions abroad, King Kojong launched a short-lived self-strengthening movement. Under China's influence, Korea concluded commercial treaties with Western powers, beginning in 1882, and opened Pusan, Wösan, and Inch'ön to foreign trade. The Western powers, however, had little commercial and diplomatic stake there. Consequently, Japan and China vied with each other for the control of Korea. But, since neither was prepared to wage war over Korea in the mid-1880s, an uneasy truce was concluded in 1885, when the Convention of Tientsin was signed by the two countries.

Deuchler's work is a diplomatic history of this ten-year period with two "focal points": Korea's relations with the treaty powers, including the creation of treaty ports and international trade; and the repercussions caused by the entry of foreign ideas and goods into Korea. It would seem that the author achieves the first of these two objectives better than the second.

Deuchler gives high marks to Kojong, not because he succeeded in becoming an enlightened ruler, but because he tried to be one. As to the significance of the trading activities of Japanese merchants in Korea, which the author describes extensively and interestingly, she merely states what has long been hypothesized: "Since the export trade was mainly in natural goods, it did not stimulate native production of manufactured goods. On the contrary, the finer quality of imported textiles may have had an adverse influence on native production" (p. 83). Concerning the failure of Korea's early modernization efforts, Deuchler assigns greater weight to internal than external factors. She writes that "the indigenous system of political and economic organization rather than the foreign challenge retarded the rapid and effective adaptation to the modern age" (p. 225). Moreover, she claims that, "whereas in Japan real drive and momentum of economic development lay outside government, in Korea economic policy was state controlled" (p. 227). This observation, however perceptive about Korea, is misleading about Japan. It overlooks the critical role played by the government in the initial phase of Meiji Japan's economic modernization.

Meticulous and thorough, the author's research is a paragon of multiarchival research. She uses a wide variety of Chinese, Korean, Japanese, and British sources. The book has an excellent index, with Chinese characters for numerous names cited. Omission of some of these names, however, could have enhanced the readability of the work without damaging its content. A more incisive analysis of fewer facts than are presented could have given the work a sharper focus. I find it rather difficult to state that the author succeeded in integrating the twin aims she set out to achieve. A minor criticism is that the author makes the common mistake of referring to nineteenth-century diplomats with the rank of minister as "ambassadors" (pp. 26, 36, 86, 114). Nevertheless, the book will well serve as an informative and useful monograph on the diplomatic history of Korea during the decade studied.

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RICHARD MAXWELL EATON. *Sufis of Bijapur, 1300-1700: Social Role of Sufis in Medieval India*. Princeton: Princeton University Press. 1978. Pp. xxxii, 358. \$25.00.

There is a considerable body of scholarly literature dealing with Sufism as a discreet Muslim religious phenomenon and with various Sufi orders as social and religious movements. There are a smaller

number of biographies of individual Sufis. And there are far fewer studies that treat Sufism as a part of the broader Islamic society. Most of the scholarly work showing the dynamics of interaction between Sufis and society at large has been done by anthropologists, such as E. E. Evans-Pritchard or Ernest Gellner; but studies of this sort tend to be limited to contemporary societies and lack sufficient historical scope. Social historians have given Sufi movements and individual Sufi masters some attention when dealing with certain regions and periods, but they have done so *en passant*. The primary focus of their interest has been directed elsewhere. Richard Maxwell Eaton's *Sufis of Bijapur* is a welcome exception.

Based upon Eaton's doctoral dissertation, it presents a multifaceted and in-depth picture of Sufis and Sufism in the social history of a Muslim city-state in medieval India. After a brief historical introduction to the region, which is not well known to general Islamic historians, Eaton traces the arrival of early, individual Sufi warriors into the Bijapur plateau during the late thirteenth and early fourteenth centuries down through the development of the Chishti, Qadiri, and Shattari orders. He goes on to describe the various roles played by Sufis under the 'Adil Shahi dynasty during the sixteenth and seventeenth centuries. The Sufis are presented as reformers, literati, landed elites, and dervishes. Nearly every chapter is capped with a convenient summary, and the entire book is neatly rounded off with a conclusion, epilogue, and three useful appendixes.

Eaton's study is based on a wealth of Persian, Urdu, and vernacular Dakhni source material, much of it still in manuscript. The author has taken a sound and judicious approach to hagiographic literature as sociohistorical documentation. His division of hagiographies into those more or less historically accurate, depending upon whether the Sufi described became the object of a saint cult or not, seems a good rule of thumb, although a little too neat.

Eaton amply proves his contention that the Sufis of Bijapur were an integral part of the society in which they lived and that their roles were far more variegated than that of humble, ascetic preachers patiently spreading the Islamic faith among the Hindu masses. He clearly shows that Sufis in India—as elsewhere in the Muslim world—differed in their styles of religiosity. "Some [Sufis] were orthodox to the point of zealous puritanism, others unorthodox to the point of heresy" (p. 283).

One of the few critiques this reviewer has to offer is that Eaton might have made more reference to parallels with Sufis in other Islamic societies, especially since he expresses an interest in comparative history. The term *dar al-Islam* ought to have been

translated "the abode of Islam" and not "the abode of peace" (p. 18), although the latter is implicit in the former. As far as this reviewer could judge, most of the Arabic and Persian terminology has been accurately translated.

Eaton is to be commended for this well-written and informative book.

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A. N. CHANDRA. *The Sannyasi Rebellion*. Foreword by JAGADISH NARAYAN SARKAR. Calcutta: Ratna Prakashan. 1977. Pp. iii, iii, 176. \$7.00.

This short monograph does little to further our understanding of the complex social phenomenon of the Sannyasi Rebellion. From about 1763 to 1800 bands roamed through Bihar and Bengal attacking repositories of wealth ranging from villages to landlord estates to revenue collection centers of the British East India Company. The term sannyasi usually refers to a Hindu wandering ascetic who may be a member of a monastic order or an individual in the last stage of his personal spiritual quest. The leadership of these raiders included both these Hindu ascetics and their Muslim equivalents, who were known as faqirs, but the bands' activities are commonly referred to as the Sannyasi Rebellion.

A. N. Chandra's thesis is that the sannyasi rebellion was a collective expression of political protest against British despotism by starving peasants, unemployed artisans, and disbanded soldiers who were led by popular religious figures. He argues that, although some attacks were directed against peasant villages, most sannyasis must have had popular support in the form of supplies and intelligence or else they would not have been able to resist the British for so many years. His conclusion characterizes the Sannyasi Rebellion as the first step in the Indian struggle for freedom from foreign rule. Unfortunately Chandra does not present evidence to support his conjectures nor does he adequately explore significant factors that would illuminate more clearly this important example of social banditry.

Chandra's narrative weaves unsupported generalizations and stereotypes into an indifferent fabric. He claims that the violence of the rebellion was encouraged by the apathy and fatalism of the Bengali people. There are repeated references to the political content of the sannyasi appeal but nowhere is that ideology specified. There is little discussion of the basis for the supposedly close ties between the peasantry and the sannyasi bands. The author seems unaware of the work of Eric

Hobsbawm on social banditry and does not ask key analytical questions nor attempt to use new sources. Chandra instead relies on a limited group of sources, mainly records of the British East India Company, studies by nineteenth-century British officials such as W. W. Hunter, and Jamini Mohan Ghosh's *Sannyasi and Fakir Raiders in Bengal*. The sannyasi bands obviously did not have a records department, but Chandra might have used local revenue records, anthropological techniques, and ballads to piece together information on the illiterate leaders and followers. Such efforts might yield some understanding of why northern Bihar and Bengal have spawned social protest movements ranging from the Sannyasi Rebellion to Muslim reform movements such as the *fara'izi* to the Naxalites in the 1960s.

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AMAL KUMAR CHATTOPADHYAY. *Slavery in the Bengal Presidency, 1772-1843*. Foreword by J. B. HARRISON. London: Golden Eagle. 1977. Pp. xv, 178. \$9.00.

The emancipation of Indian historiography, from apologetic British literature on the one hand and Hindu writings of self-glorification on the other, has been a gradual process since political independence in 1947. On one end of the polarity, historians praised agents of Company and Empire for having bestowed the blessings of Westernism under the providential and benevolent rule of the British. On the other end, Indian nationalists such as Gandhi blasted the very foundations of modern Western civilization, while at the same time concealing indigenous shortcomings and abuses in an effort to mobilize cultural pride and integrity. It should come as no surprise, therefore, that the idea of Hindu renaissance was perhaps the major historiographical contribution among Indian writers of history in the century before independence. The Indian elite under British colonialism responded positively to renaissance as the reconstruction of a remote golden age, the creation of a new tradition of reformed Hinduism, and the birth of an ideology of national liberation.

Amal Kumar Chattopadhyay's *Slavery in the Bengal Presidency, 1772-1843* represents the new post-independence Indian historiography, which is largely anti-renaissance, anti-elitist, anti-urban, and anti-nineteenth-century heritage. Especially in West Bengal, presently under a Communist government, Marxism and Maoism have deeply influenced the historical consciousness of the intelligentsia. Totally disenchanted with the *bhadralok* or bourgeois values of past generations, younger Bengali historians have rejected renaissance

heroes, nationalist myths, and the Calcutta-centric view of Bengal past and present. Instead, they have innovated studies of the underprivileged rural masses exploited from British times not simply by foreign planters but by Indian landowners known as *zamindars*. Agrarian history is certainly the most exciting new field in Bengali historiography, and it is precisely in this context that Chattopadhyay's study of slavery belongs.

The book, originally a London University doctoral dissertation, is not a propaganda piece but is ably written and amply documented from court records, official correspondence, personal collections, revenue consultations, and the wealth of archival material at the Indian Office Library. After defining "slave" in the classical Aristotelian sense as a "living tool, a tool which can move and talk with some amount of intelligence," and slavery as "the ownership and use of human property" (p. 1), Chattopadhyay briefly surveys the institution among Hindus and Muslims before the advent of the British. Having established the fact that slavery was an integral part of the Hindu and Islamic civilizations, he then organizes his eight chapters around three broad themes: the modification of slavery in Bengal Presidency under East India Company policy, the external slave trade in Bengal, and the British antislavery movement whose agitation helped eradicate the evil finally in 1843.

For an original work, the monograph is unusually well organized, comprehensive, and detailed. Chattopadhyay covers slavery and caste, the slave trade and children, slavery and prostitution, slavery and famine, domestic slavery, slavery and marriage, agrestic slavery or "the use of unfree labor for the cultivation of the soil" (p. 58), plantation slavery, and slavery by the Europeans. His final two chapters, which examine British attitudes for and against slavery in India, are very useful to social and intellectual historians interested in the nineteenth-century idea of slavery in cross-cultural perspective.

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JOHN R. MCLANE. *Indian Nationalism and the Early Congress*. Princeton: Princeton University Press. 1978. Pp. xii, 404. Cloth \$25.00, paper \$12.50.

B. R. NANDA. *Gokhale: The Indian Moderates and the British Raj*. Princeton: Princeton University Press, 1977. Pp. x, 520. \$25.00.

Why did the moderates dominate India's nationalist movement from 1885 through the First World

War, and why was the early nationalist movement so unsuccessful in bringing Hindus and Muslims together? These two fine accounts of the early history of the nationalist movement, the one focusing on the Indian National Congress from its founding in 1885 to 1905, and the other on the political career of India's leading moderate from 1889 to his death in 1915, give us as clear an answer to these questions as any studies published thus far. Both are well written and well documented. B. R. Nanda's is in the best tradition of political biography, while John R. McLane's study is informed more by the new school of historical scholarship that focuses on the relationship between local social and political forces and national politics.

Gopal Krishna Gokhale was a leading figure in the nationalist movement at the turn of the century, president of the Indian National Congress, founder of the Servants of India Society, and a Gladstonian liberal. He was India's leading parliamentary critic of the British and a major figure in the struggle for constitutional reforms to extend self-government to India.

Gokhale was also a social reformer. He lamented the lack of discipline in India and the weak public life. He denounced untouchability, was an advocate of female education, supported widow remarriage, and was in favor of restricting the age of marriage. Like many social reformers of his time, Gokhale wrote that Western education would "liberate the Indian mind from the thralldom of old-world ideas" (Nanda, p. 184).

Two groups were opposed to Gokhale. In his usual lucid manner, Gokhale described them: "On one side was the bureaucracy, a small body of foreign officials, who held in their hands practically a monopoly of all political power. . . . On the other side . . . the vast mass of the people of the country lying inert and apathetic, except when under the sway of a religious impulse. . . . Between the two there stood the educated class with its number steadily growing, already exercising extensive influence over the mass of the people" (Nanda, p. 262).

Nanda argues that the moderates were not merely a small, urban, educated minority concerned with its own class interests. "The Moderates," he writes, "looked upon the British empire . . . as a valuable link with the western world for those cultural, technological and commercial interchanges which were essential to usher India into the twentieth century" (Nanda, p. 485). The extremists who opposed Gokhale and the moderates were prepared to use violent and even terrorist means to dislodge the British; they were also very much opposed to Gokhale's vision of India as a secular, modern society. Gokhale's dream of a self-

governing, democratic India was also a nightmare to India's Muslim leaders, who rejected the notion of an Indian nationality and who feared that a competitive civil service and representative institutions would mean Hindu domination. And although Gokhale dreamt of bringing the Hindus and Muslims together into a common nationality, the Hindu orthodox had no such illusions.

McLane's book provides a particularly rich description of the Hindu orthodox opposition to the social reformists and to the Muslims. In one of the most interesting sections of his book McLane portrays the rise of the Hindu movement for cow protection and its impact on both nationalist and Muslim politics. His central theme is that the cow was a symbolic defense of Hindu values and customs against both foreign domination and Indian social reformers. The north Indian cow protection riots of 1893 resulted in a sharp decline in Muslim participation in the Indian National Congress, reflecting the growing anxieties of India's Muslims toward the nationalist position. McLane does not ignore the role played by the British in dividing the Muslims and Hindus, but his emphasis is on the increasing vitality of Hindu and Muslim religious forces at the turn of the century and the way in which the Westernized middle classes and especially the moderates were caught between forces they could not control. It was the Hindu revivalists, with their emphasis on Hindu identity and their more aggressive posture toward the British, who succeeded, as the moderates had not, in stirring up popular participation in the nationalist movement. And, as the movement became more populist, as it responded to Hindu opposition to the partition of Bengal, denounced the purchase of British manufactured goods by Indians, and attracted those who favored banning the slaughter of cows, the nationalist movement increasingly alienated the Muslims.

McLane thus brilliantly portrays the political dilemma faced by the moderates. Their commitment to social reform led them to support the Age of Consent Bill and to oppose the ban on cow slaughter, but their position on these issues often alienated them from their own families and communities. McLane suggests that, in an effort to strengthen the nationalist movement, the moderates attempted to win the support of the landlords in Bengal, Bombay, and the Punjab by defending the permanent settlement and by taking a cautious position on all agrarian reform questions. He concludes that the moderates preferred to pursue a strategy of trying to reduce conflict between the *zamindars* and the urban middle classes rather than trying to attract the support of "an apolitical peasantry" (McLane, p. 267).

In short, the moderates wanted to achieve self-

government through constitutional measures without the mass organization of the peasantry, for they believed that the cause of social reform was being damaged by the extremists stirring up the "religious impulse" of the masses. But even as Gokhale and the moderates were at the peak of their power within the nationalist movement, forces were at work to create a new era of mass politics. The moderates were eventually displaced by new leaders but they left behind a commitment to secularism, constitutionalism, democratic nationalism, and social reform that continues to hold an important place in contemporary India.

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LAURISTON SHARP and LUCIEN M. HANKS. *Bang Chan: Social History of a Rural Community in Thailand*. (Cornell Studies in Anthropology.) Ithaca, N.Y.: Cornell University Press. 1978. Pp. 314. \$17.50.

Bang Chan first appeared as a small hamlet settled by migrants from the Thai capital, Bangkok, in the 1850s. As new migrants joined the early settlers, the community grew, experiencing alternate periods of prosperity and economic depression. Finally, in the 1950s and 1960s, Bang Chan was absorbed into the expanding suburbs of Bangkok. This history of Bang Chan is intended to be a study of social change and development in rural Thailand. It is also a personal tribute to the inhabitants of the community, for whom the authors feel great affection.

The text of the study is composed of a montage of excerpts from interviews with Bang Chan's residents made in the late 1940s and early 1950s, quotations from the accounts of Western visitors to Thailand, and the comments of the two authors. The oral sources are the most important and the most valuable part of the book, but, because of the continuous movement of people into and out of Bang Chan, the oral record is incomplete. Although the observations of Western visitors help to fill out the narrative, their remarks refer to the overall appearance of the Thai rice plain and to the general activities of its inhabitants, not to the specific community of Bang Chan. As a result, Bang Chan, with little identity of its own, merges into a very generalized landscape. There is also a tendency for the authors to project the present back into the past, a tendency shown most clearly in the discussions of family and social relationships, religion, and agricultural customs. Important social and technological changes, however, are recorded in chronological sequence: the freeing of slaves, the competition for land in the 1890s, and the appearance of machines. Consequently, the

social history can be confused. Social and technological development do not appear to have had any impact on kinship patterns, social relations, or religious beliefs and practices, yet major changes must have affected society, possibly in ways that the authors and their assistants did not record.

The decision of the authors not to refer to written records, a decision made because of a shortage of manpower and of time, limits the value of the book. Written records were obviously available, as the authors refer to land deeds, court records, and the notebooks of headmen; there were also regional records in the National Archives and the National Library. An examination of these materials would have added to the limited oral information about Bang Chan's past, and would have provided a basis for comparing with and checking the oral record. Even where written records are inaccurate, as land records often are, they can still provide reliable information about the social groups who make use of the record system, relations between officials and the members of the community, and concepts of land ownership and land transfer.

CONSTANCE M. WILSON

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DOUGLAS PIKE. *History of Vietnamese Communism, 1925-1976*. (Histories of Ruling Communist Parties.) Stanford: Hoover Institution Press. 1978. Pp. xiv, 181. \$5.95.

Since the publication of his first book, *The Viet Cong* (1966), Douglas Pike has emerged as one of the foremost Vietnam experts in the United States. While his role as a defender of American policies in Southeast Asia during the 1960s exposed him to criticism in academic circles, he has written frequently and well on various aspects of Vietnamese Communism. He was thus a natural choice to write the selection on Vietnam for the Hoover Institution series on the histories of ruling Communist parties.

The task is no easy one, for in Vietnam the history of the party is deeply imbedded in the history of modern Vietnamese society as a whole. At the outset, Pike emphasizes that his book is intended solely as a history of the Vietnamese Communist Party since its founding in February 1930 and not of the generation of conflict leading up to the fall of Saigon in 1975. In general, he adheres to his plan. While alluding occasionally to the broader context of half a century of revolution, he concentrates upon the origins and growth of the party as an organization. Special emphasis is placed on the leadership, and brief biographies of the major figures in the history of the party are

included. The treatment of the multitude of issues in the life of the Communist Party is well balanced and even handed. Pike has never hidden his distaste for the Communists in Vietnam, yet for the most part he appears willing to give credit to the party for its achievements where such credit is due.

If there are any weaknesses in this book, then, they are weaknesses of omission rather than commission. Vietnamese Communism (even when construed in a narrow sense) is an immense and complex topic, and a spate of books and articles has appeared on it in recent years, in Western languages as well as in Vietnamese. Unfortunately, Pike's study is too brief to permit him the intensive analysis that the subject deserves. While his analysis is competent, it is sometimes superficial and lacks the insights that we have come to expect from an observer of his caliber and experience. He has not consulted the colonial archives in Paris, a major source of information on the early history of the party. Nor, curiously, has he made use of recent publications on the party produced in Hanoi, despite his comment that, on the whole, such materials are "informative and reliable."

For these reasons, Pike's new book may be somewhat of a disappointment to the specialist. It offers relatively little that is new to those already familiar with the history of the party, and will not serve as the definitive history of the rise of Vietnamese Communism that we have been waiting for. In the meantime, Pike's slim volume will be useful as an introductory text and a handy reference for specialists interested in the history of the party.

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NOEL RUTHERFORD, editor. *Friendly Islands: A History of Tonga*. New York: Oxford University Press. 1977. Pp. xii, 297. \$36.00.

Tonga tempts the imagination with the romantic allure of a kingdom that, alone among the colonized and recolonized Pacific islands, retained its sovereignty. But as for the promise that Tonga retained its cultural authenticity with its nominal independence, or found uniquely Tongan solutions to development of a viable island economy, or escaped the economic ills that have beset the less fortunate foreign-dominated islands . . . alas! Noel Rutherford's *Friendly Islands: A History of Tonga* destroys its own illusion.

Tonga protected itself from the onslaught of manifest destiny by opting for the trappings of "Western Civilization" before it could be pronounced "savage" and by adopting a Europeanized form of parliamentary government, European manners and mores, finances and finery before

these could be imposed from without. Thus the supposed independence of the Kingdom of Tonga was really a conceit perpetrated by English advisers to the throne. Traditional noble titles, rituals, clan fealty, and feast obligations remain more deeply embedded in such places as Yap and Ponape, where colonialist regimes have largely ignored traditional systems in imposing external order. Cultural conservationists advocating bilingual-bicultural projects throughout the Pacific should take heed of George C. Marcus: "The early introduction of widespread literacy in the Tongan language has not aided the preservation of old cultural remnants, but has likely accelerated culture change and assimilation in the direction of the European institutions through which literacy spread" (p. 213).

The most colorful and highly charged chapters of the book describe the political and economic intrigue, the intense rivalries among ship captains and missionary factions hell-bent to save Tongan souls; the motivation for persuasion to one faith or another was clearly a power struggle among titled Tongans. This phenomenon of conquest by Christianization occurred throughout the Pacific, and the accounts by Sione Lātūkefu and Hugh Laracy are particularly well done.

This history attempts the difficult merger of oral tradition—the "knowledge, myths, and legends . . . transmitted orally from generation to generation," criticized as factually "laconic" and "inadequate" in terms of dates and details (Jens Poulson, pp. 8–9)—with what I would call "Western history"—scholarly, fully adequate in terms of dates, names, and events. This amalgam is further synthesized with findings from archeology, anthropology, and economics; the outcome is harmoniously congruent and uniformly well written. Noel Rutherford has blended the efforts of ten learned persons with precise and cautious attention to fact, detail, and style. Yet the result is disappointingly synthetic, devoid of a sense of the culture and humanity of the Tongans as a people. Even the indigenous history has been so "Europeanized" by its scholarly filter that we are denied the warmth, the aura of grandeur, and the anecdotal wit with which folklore enhances its persona. Once the first maritime encounter is described, *Friendly Islands* slips back into the traditional mold—it becomes not the history of Tonga but the history of Europeans in Tonga and, of course, Europeanized Tongans. The laudatory chapters concerning the King Georges and Queen Salote were written with a Western academic bias, when surely island oral tradition abounds among the less educated, or less scholastically inclined, element of Tongan society.

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UNITED STATES

GARRY WILLS. *Inventing America: Jefferson's Declaration of Independence*. Garden City, N.Y.: Doubleday and Company. 1978. Pp. xxvi, 398. \$10.00.

I have never struggled with a more perverse book. Its strengths justify all the effort, while its faults insure intense frustration. Garry Wills has much to teach an audience. But he so often misinforms that one soon doubts any net gain.

Wills's subject is not the Declaration of Independence. Except in an opening section, he adds little to our understanding of that public document. His tasks are to infer what Jefferson meant in the words of his original draft and to correct what he sees as later misuses of the document. As Wills understands him, Jefferson often held private or eccentric meanings for the language of the preamble. But even Jefferson's Declaration does not always remain in focus, for at times it provides Wills only a convenient entrée into biography; he has been pulled into the old search for the "real" Thomas Jefferson. Despite the depth and subtlety of his insights, despite his often ungenerous but telling attacks on earlier historians, his final product is as self-revealing and as sharply colored by present concerns as any I have ever read.

Wills is at his best on narrow, clearly focused issues. At times his is a virtuoso performance, with sharply honed arguments backed up by an onslaught of scholarship. Even his asides are often pure gems. He brilliantly paints the setting of the second Congress and clarifies well the specific grievances against Britain. No one has more abundantly and convincingly documented the influence of Francis Hutcheson and other Scots on a youthful Jefferson, but so often Wills seems unaware of larger continuities. His comparative references are generally mistaken or textbookish. For example, he builds much of his central argument around such a simplistic caricature of "Lockean orthodoxy" as to embarrass any sophomore. He has no better understanding of late medieval political theory than did Jefferson. This blindness does not preclude acute inferences about Jefferson's beliefs, but almost always distorts their historical position and role.

Wills's Jefferson was an admirable fellow, given the moral preferences of a Wills. That is, Jefferson rejected an acquisitive "Lockean" individualism and never had an overweening concern for private property. He was a humane, excessively sentimental, public-spirited man, with a keen sense of communal responsibility. Perhaps above all, he embraced an extreme form of equalitarianism. As a portrait of Jefferson, this is no worse than many predecessors. I suspect it overemphasizes certain

facets of a complex man and withal makes Jefferson both a more consistent and a more prophetic thinker than he really was. But Wills at least does a service to historical truth by correcting some easy and still prevalent stereotypes (such debunking often seems the prime goal of his book). He makes clear that neither Jefferson nor his Scottish mentors, up to Adam Smith, ever lacked a strong sense of communal responsibility. Images of selfish individualism, of inordinate concern for private acquisition, and of "capitalist" greed simply do not fit them.

Yet, such a "corrected" emphasis distorts. It leads Wills to a tortured and strained reading of two critical doctrines—unalienable rights and human equality. Wills conveniently ignores most of the critical ambiguities in such words as "rights" and "property." The inherent rights of traditional natural law theory, the rights so conventional as to be commonplace by 1776, involved a moral claim tied to individual identity. By the eighteenth century this sense of a right (the opposite of a wrong) mixed with a more functional or governmental sense—a right as a socially justified leeway for private action. Wills ascribes this emerging, almost utilitarian meaning to Jefferson and in this way opens the door to a very social or corporate conception of rights.

Consistent with this functional understanding, Wills argues that Jefferson calculatingly left property out of his trilogy of rights. He thus rejected "Lockean" individualism. This may sound plausible to modern ears, but it is just not so. It inverts the pattern of historical development. Jefferson, unlike Wills, did come to a belated understanding of many of the ambiguities in the word "property." He saw the need for limits on ownership even as he worked in behalf of easier access to land. But he never questioned the moral legitimacy of possessions gained through labor or of fee-simple ownership of land. Neither he nor his Scottish mentors placed as many qualifications around these forms of property as did Locke or his Puritan predecessors. Jefferson simply had less of a sense of need for communal restraint and wanted greater leeway for individual acquisition and free exchange. In this sense Jefferson and Adam Smith were part of developments that led to increased economic options for individuals and away from seventeenth-century communal priorities.

Locke often used the word "property" in the widest possible sense. But for material possessions he found an objective moral justification only for possessions inescapably assimilated to personality; that is, for products of immediate labor or, in the absence of scarcity and to the limits of need and use, for enclosed land. By implication, his views necessitated common ownership of land in a scarce

environment, a position far beyond any advocated by Jefferson. Of course Locke supported other forms of ownership on the basis of universal consent or positive law, but these have nothing to do with unalienable rights. We still do not know why Jefferson left property out of his preamble, but nothing suggests that the omission entailed any subordination of these special forms of property to the fully complementary and inseparable rights of life and liberty. It is good to remember that later radicals, such as Thomas Skidmore, attributed the weak euphemism, "pursuit of happiness," to cowardice, to Jefferson's unwillingness to embrace the full and hard challenge of property rights—the duty of government to secure to each person his birthright in nature.

On the issue of equality, Wills flirts with sophistry. He argues that Jefferson went well beyond the standard juristic meaning of "all men are created equal." First in Virginia, and then in most state declarations of right, the meaning was much clearer: "all men are created equally free and independent." Such a meaning for equality grew out of late medieval attacks on slavery and made clear that any morally acceptable servility or political obligation rested not on God's will, not on birth, not on levels of ability, but on consent. Wills argues that Jefferson meant much more than such juristic equality, that he believed individuals all but equal in virtue if not in ability. His perceptive analysis of Jefferson's moral sense theory never substantiates this argument. Jefferson believed everyone had a moral capacity, but one which matures only under favorable circumstances. All earlier versions of equality also departed from the belief that all men are morally accountable. Jefferson's view would have been more radically equalitarian only if he had believed that all people were equally moral, which is a problem of behavior and not of potentiality.

Wills tries to use the moral sense doctrine to dignify Jefferson's racial beliefs. The doctrine of juristic equality, whether tied to a special moral faculty or to other ethical theories, made clear the tyranny of involuntary servitude but still left room for wide differences in individual ability and achievement. So did Jefferson. He always condemned slavery as immoral. Blacks were fully human and possessed all human rights. At times, as Wills so effectively argues, Jefferson even speculated that they excelled whites in certain virtues of the heart. But empirical observation usually persuaded him that blacks were inferior to whites in reasoning ability. Even here he had enough scientific caution to leave such an empirical issue open for more careful testing. Surely intelligence and not the essential humanity of blacks has been the racial issue in American history. If so, then none of

Wills's tortured arguments about real equality alters by one whit the traditional understanding of Jefferson.

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CHARLES O. JACKSON, editor. *Passing: The Vision of Death in America*. (Contributions in Family Studies, number 2.) Westport, Conn.: Greenwood Press. 1977. Pp. xi, 258. \$17.95.

This is a collection of fifteen "pieces"—no other word quite suffices—on various aspects of death in American history. These pieces, which range from scholarly essays to snippets from books of widely varying importance, are organized into three sections and an epilogue written by the editor. The three sections represent three historical periods: the colonial era, the nineteenth century, and the twentieth century; each section contains one primary document and one comment specifically on "the cemetery as a cultural institution"—a singularly unoriginal designation. But then, not much is original here. The editor claims otherwise. He asserts in his introduction that this volume differs from other anthologies because it "has been developed around an overall thesis." Such a claim of uniqueness may surprise other editors. But in any case, what Charles O. Jackson's "thesis" amounts to is this: between the late eighteenth and late nineteenth centuries the image of death was "domesticated" and "beautified"; since the late nineteenth century (with the present representing some difficulty for him) there has been a steady "withdrawal on the part of the living from communion with and commitment to the dying and the dead."

That's it: pretty much what has been said, and known, and said again for the past two decades. It is not surprising that the book's table of contents reads like an undergraduate course syllabus. But—and here is where *Passing* (despite that euphemistic and physiologically ambiguous title) may be of some value—it reads like a good undergraduate course syllabus, a syllabus listing standard but not always conveniently accessible "pieces." Among these are: a section of Cyclone Covey's interesting little book, *The American Pilgrimage*; a chapter from one of Alice Morse Earle's vintage volumes; the important essay, "Death's Heads, Cherubs, and Willow Trees," by Edwin Dethlefsen and James Deetz; some pages on the rural cemetery from Neil Harris's *The Artist in American Society*; Robert Blauner's fine essay, "Death and Social Structure." In addition, the book contains a more than adequate bibliography for undergraduate reference.

All of this leads to one conclusion. Although not of much scholarly value, *Passing* could be useful in the classroom should the publishers decide to issue it in inexpensive paperback form.

DAVID E. STANNARD
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CLIVE BUSH. *The Dream of Reason: American Consciousness and Cultural Achievement from Independence to the Civil War*. New York: St. Martin's Press. 1977. Pp. xi, 397. \$28.00.

His book, Clive Bush asserts, is an experiment which for its "organizing pattern . . . is deeply influenced by the central chapters of Norman O. Brown's famous work, *Love's Body*: 'Person,' 'Representative,' 'Head,' and 'Boundary' " (p. vii). His own purpose is to examine some of the common themes of American Studies as a whole, rather than separately, and thus to write a history of American consciousness and cultural achievement from Independence to the Civil War. The argument is complex in ways which unfortunately have become synonymous with much of the genre of American Studies. Bush, however, utilizes his considerable knowledge of American literature and painting to offer some insights and interpretations which should prove interesting to historians. Since conventional intellectual history is not his concern, he may be justified in ignoring most of the major work in this area in favor of a bibliography heavily weighted with older titles and art history. More attention to recent historical studies might, however, have helped him achieve a clearer, more understandable synthesis of his bewildering and allusive diversity of sources.

Part one, entitled "Civil History," carries forward the underlying theme of a decline of the dream of reason in comparison with a rising romantic nationalism. Beginning with the Puritans' adoption of Peter Ramus's work, Americans were concerned with their European legacy, alternately modifying or rejecting Old World theories of culture and society. Chapters on the "Hero" and the "Arts of Peace," in contrast to the "Arts of War," delineate this process in literature and painting. In his discussion of the arts of war, Bush analyzes the symbolic roles of West Point and of Samuel Colt's military-industrial philosophy. "The years between 1776 and 1860," he writes, "see the progressive failure of a popular antimilitarism (the individualist sentiment) and the proportional growth of a centralizing, military-aggressive, state bureaucracy" (p. 120).

Before turning to "Natural History," the subject of part two of his book, Bush rather paradoxically offers an account of the invention of photography which, he contends, challenged the whole previous

nature of representative reality. His own treatment of natural history, nevertheless, covers familiar ground, beginning with the three "literary geographies" by Jefferson, Crèvecoeur, and William Bartram. Government exploring expeditions and the American treatment in literature and the visual arts of the North American Indians, animals, birds, and landscape complete Bush's account. Of the Americans who attempted most seriously to understand these important subjects, he devotes special attention to John James Audubon and Herman Melville, along with such contemporaries as George Catlin, Thomas Cole, James Fenimore Cooper, Currier and Ives, and Charles Wilson Peale. The variety as well as the richness of their achievements made it possible for America in the first half of the nineteenth century to "resist both the European artistic legacy and the acquisitive, antiintellectual, business-oriented, expansionist and materialist cultural bias of the new republic's own making" (p. viii).

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ELIZABETH FLOWER and MURRAY G. MURPHEY. *A History of Philosophy in America*. In two volumes. New York: G. P. Putnam's Sons. 1977. Pp. xx, 435; 463-972. \$15.00 each.

Since its publication in 1946, Herbert W. Schneider's *History of American Philosophy* has been the standard comprehensive treatment of its subject. Several fine works have explored American philosophy thematically or selectively, but it is to Schneider's volume that the new work by Elizabeth Flower and Murray G. Murphey most readily bears comparison. Although both efforts are broad syntheses, they are cast in clearly different molds. Schneider viewed philosophy expansively. He embraced the fields of political and economic ideas as well as popular religious movements. His chapters on "Whig Nationalism," "The Common Man," "Young America," "Idealistic Democracy," and others indicate the broad domain of public ideas that interested him. Schneider's work, in large measure, really constituted an intellectual history. But the two volumes by Flower and Murphey are a philosopher's history of philosophy. They are at once far more technical in language and more exhaustive in treatment of intricate philosophical problems. Flower and Murphey's fifteen chapters are organized either by themes or persons and groups, and within each of these they have attempted to present extensive analyses. On its own terms the new history is most impressive, almost a monumental achievement.

Space affords only a limited opportunity to high-

light the main themes and insights of this study. The authors wisely refrain from distilling an "essence" of American philosophy but refer in an introductory note to the mediating role that philosophy has played between science and religion in America. They do not press the matter in a heavy-handed way, but the theme stands forth prominently throughout their discussions. We see it clearly in Puritanism, then with fascinating variations in Cotton Mather and Samuel Johnson in the Enlightenment. It is manifest, of course, in Jonathan Edwards, but more subtly at work even in the materialistic philosophy of Thomas Cooper, in Peirce's ultimate recourse to a Universal Mind to complete his evolutionary cosmology, and even in Santayana's dissolution of the warfare of science and religion in his attempt to portray each as merely different uses of the human imagination.

The authors follow traditional form in introducing American philosophy in an excellent chapter on Puritanism. But their work gives greater recognition than others to the enormous impact of Scottish philosophy in America. Most writers have confined this impact to academic philosophy, and Flower and Murphey, although not uncritically, treat that issue with a focus on Princeton, from Witherspoon to McCosh, the "immensely influential president" of that college. But the Scottish connection was greater yet. It made possible a significant shift away from Puritanism by making ethics an empirical science. It so tempered the American frame of mind as to affect critically the reaction to Kant, helping to prepare the way for the later pragmatic theories of mind and diluting the neo-Hegelian reading of the great German that predominated in England. In logic the influence of Reid was decisive, breaking from the Aristotelian concern with the formal structure of knowledge to the psychological structure and operations of the mind.

The Scottish presence was also an important factor in shaping a philosophical style in the Middle Atlantic and Southern colonies. The authors have much respect for the New England tradition (further illustrated in a fine chapter on Edwards) and its influence, but they have discerned some impressive contributions elsewhere. To the south of New England a scientific-materialistic atmosphere prevailed, effecting its own kind of mediation of science and religion. The important chapter that explores this theme brings renewed appreciation of Francis Allison, William Smith, Joseph Priestley, and especially Samuel Stanhope Smith of Princeton. Here also, with the help of French sources, Cooper and Jefferson explored the physiological basis of knowledge and anticipated the later achievement of William James.

A distinguishing feature of this history is the

attention it gives to the subject of logic. The chapter on Puritanism contains an extended section on the system of Ramus, and later an entire chapter explores the advancement of logic in New England. Here the Scottish philosopher Sir William Hamilton receives special appreciation; his revolutionary achievements were recorded in America by Francis Bowen at Harvard. Also, within several chapters that explore the thought of individual philosophers—Peirce, Royce, Dewey, and C. I. Lewis especially—studies of inference, the logic of relations, and the categories of thought receive extensive treatment.

It is in these sections too that most readers will find this history difficult and often trying. The language is extremely technical, the ideas clearly addressed to readers with more than a general familiarity with the ideas and terminology. (The reader uninformed of Russell-Whitehead numbers and Peano axioms, for example, will get no help from the authors.) Unfortunately, the formalistic style of these sections spills over into much of the work. The writing is often cramped and arid, and many parts of this work will engage the interest and excitement of only the most experienced philosophical readers. And nowhere is this problem better illustrated than in the unfortunate chapter on John Dewey. Most of it is an exploration of Dewey's theory of inquiry and his writings on ethics and valuation. Dewey's most widely read works, *Democracy and Education*, *Reconstruction in Philosophy*, *Art as Experience*, and *A Common Faith*, are not cited, and a brief section on Dewey's social thought wholly fails to illustrate the immense social and political implications of his revolt against the Greek metaphysical tradition in Western civilization. Indeed, one can read this chapter and fail completely to appreciate how Dewey's career in philosophy influenced thousands of Americans.

Nonetheless, in what the authors attempt to do, they do exceedingly well. Certainly no other work of its kind shows such a sensitive and critical mastery of the contours of philosophical thinking. Furthermore, no similar work affords the reader so many comprehensive reviews of American philosophers. The chapters on Edwards, James, Royce, and C. I. Lewis are not merely general reviews but lengthy and extended analyses. Those on the St. Louis Hegelians and the Darwinian controversy are also highly useful. Flower and Murphey have produced an insightful synthesis of American philosophy and a valuable reference work as well.

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S. RUFUS DAVIS. *The Federal Principle: A Journey Through Time in Quest of a Meaning*. Berkeley and

Los Angeles: University of California Press. 1978. Pp. xi, 237. \$11.50.

S. Rufus Davis, a political scientist, has written a useful historical account of the evolution of the concept of federalism that includes an assessment of the significance of the concept in contemporary political life. After reviewing ancient, medieval, and early modern writings concerned with combinations of states into larger governing units, Davis analyzes the establishment of federalism in the United States. On the basis of this central event in the history of the concept, he defines federalism as the principle of government that provides within a single constitutional system two distinct governing authorities, each of which acts directly on individuals and possesses qualified sovereignty. This point is easily enough grasped, and Davis quickly goes beyond it to dwell on the many questions about federal-state relations that were left ambiguous in 1787 and that remained so well into the nineteenth century. He aptly observes that the Constitution grafted two centers of authority into a union of unfamiliar and unpredictable relations and concludes that the language that described the states as "co-ordinate" or "co-equal" with the federal government was but a formal symbol of political accommodation.

In the second half of the book Davis considers nineteenth- and twentieth-century variations of federalism patterned on the American model. As in earlier chapters, he analyzes leading writings on federal organization, but here his purpose shifts to a political scientist's evaluation of theories of federalism. Is it possible, he asks, to formulate a descriptive, explanatory, and predictive theory of federalism, or is there no theory at all but only the history of particular federal experiments? Criticizing the theories of federalism advanced by Riker, Wheare, Friedrich, and others as ambivalent, uncertain, and excessively general, Davis seems to say that theory has failed, that there is only history. And to those who, seeing federalism in decline, look for its revival, he offers the pessimistic conclusion that integration and centralization have all but destroyed federalism as an effective system of government. Davis nevertheless contends that the federal symbol will continue to exist and exert a positive influence on political life.

Although the principal historical meaning of federalism has been divided sovereignty between two or more governments, the essential political and moral meaning that Davis sees in the federal symbol is promise, commitment, or covenant. These terms are signified in the linguistic root *foedus*, from which we derive the word "federalism," and because commitment and covenant are the basis of all forms of association and coopera-

tion, Davis believes the federal symbol will continue to have practical value in politics. In other words, although the irresistible hegemony of central government has rendered theories of divided sovereignty all but irrelevant as an effective form of political organization, the federal concept can still serve as a symbol of men's desire to come together to promote personal and communal welfare.

As an historical essay on the federal idea and as a critique of contemporary federal theory Davis's book warrants the attention of constitutional historians. It is difficult to agree, however, with Davis's conclusion that the essence of the federal idea—the covenanted association of *foedus*—will continue to motivate and shape political action if the form of federalism—divided sovereignty—is superseded by centralization. One can think of political forms lingering after their inner spirit has disappeared or has been transformed, but that an animating principle or idea should continue to influence events after its historic institutional expression has ceased to exist seems doubtful. If the constitutional future indeed belongs to centralized government, as Davis seems to say, and division of authority or divided sovereignty ceases to describe actual governmental practices, then there would appear to be no basis for invoking or appealing to the federal symbol.

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VINCENT A. LAPOMARDA. *The Jesuit Heritage in New England*. Worcester, Mass.: College of the Holy Cross. 1977. Pp. x, 321.

Casting an eye to the nationwide bicentennial observation, Holy Cross College has published this commemoration of Jesuit activity in New England from the colonial period to the present. Calling his book an introductory survey, Vincent A. Lapomarda attempts to spread "knowledge of the people and events . . . landmarks, historic sites and places of interest" touching upon the history of the Jesuit Order in New England (p. viii). The author divides his material into geographic areas and then discusses each region with reference to three periods: colonial, nineteenth century, and twentieth century.

Lapomarda has gathered information from archives and an impressive array of published sources. In the seventeenth and eighteenth centuries Jesuit priests arrived from France for missionary work among the Canadian tribes. As these missionaries ranged south, they made frequent and often friendly contacts with settlers on the New England frontier. The Jesuit Order, sup-

pressed by papal edict from 1773 to 1814, emerged in the nineteenth century to assist New England bishops in missionary and educational projects. Lapomarda describes the early flourishing of Jesuit schools and colleges, as well as the praise and opprobrium gathered by the order in Protestant America. Covering the events of the twentieth century, the author sketches the maturation of Jesuit education and points to its influence in the New England area.

Lapomarda's effort to record and celebrate four centuries of Jesuit activity blurs the focus of his book. Minor events and coincidences obscure the narration, while the various chapters have only loose ties to one another. Moreover, the study lacks the analysis demanded by its scope and detail. The work would have achieved greater coherence by attempting to define the role and contribution of the Jesuits in both New England and the Catholic church.

The historical study of the American Catholic church remains relatively undeveloped despite the church's educational and intellectual resources. In his preface Lapomarda expresses the hope that his book will prompt further investigation. To facilitate this, his bibliographical essay provides detailed guidance to both published sources and archival materials. Coupled with this bibliography, Lapomarda's overview sketches general lines for future research in this still unexplored field.

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FREDERICK RUDOLPH. *Curriculum: A History of the American Undergraduate Course of Study Since 1636*. San Francisco: Jossey-Bass Publishers, for the Carnegie Council on Policy Studies in Higher Education. 1977. Pp. xiii, 362. \$12.95.

In the foreword, Clark Kerr of the Carnegie Foundation writes that this book was commissioned in the belief that historical analysis can explain both the origins of and the tensions between different curricular models for undergraduate college education. Indeed, Frederick Rudolph recognizes that controversy about the curriculum can seldom be read at face value. The conventions of his own discipline, nevertheless, confine him as he retells the story of the American college from the seventeenth century to the twentieth. It is an account rich in particulars about notable persons, institutions, events, and historiography. Rudolph consistently enlivens the narrative with both perceptive observations and provocative judgments, especially in criticism of the disarray caused by the marketplace of electives and by university professors disinterested in teaching college students. His

assessment of historical trends tends to be gloomy. General education, lectures, honors—few of the twentieth-century remedies against the expansion of the curriculum to the verge of chaos have effectively restored the common learning and authority that once defined the educated man. Lacking any internal symmetry or coherence, the curriculum in the last half-century has turned into a bureaucratic notion of convenience, an administrator's idea rather than a teacher's.

Rudolph's book is the first historical survey of the undergraduate curriculum in many years, and for good reason. First, the subject suffers from an excess of both platitudes and minutiae. It has been lost in endless controversy, burdened by clichés about educating good, interesting, and wise people, and often so fragmented by institutional accommodations as to defy synthesis. Second, the subject generates little intrinsic excitement, making it especially elusive. When selecting an institution to attend, students have not traditionally concerned themselves greatly with the list of courses in a catalogue. Moreover, both academics and administrators have tended to manipulate the curriculum in order to further their own special interests. For instance, a department consolidates a power base by dictating required courses and a college president establishes his own identity by supporting general education against the narrow professionalism of the faculty.

One only wishes that Rudolph would have probed the topic more deeply. In a book entitled *Curriculum* why did the author not discuss the historical origins, meanings, and usages of the word itself? It may be misleading to use the word as freely in the context of colonial America as in the context of the present. The Scots first employed the word, but, in fact, in America B. H. Hall did not record it in his collection of college words (1856), though the earliest dictionary appearance I uncovered was in an edition of Webster in that same year. Noah Porter, for instance, never made the word central to his vocabulary, and the Century dictionary in 1889 still did not include "curriculum." After all, curriculum is a concept, and because root meanings related this concept to a horse race and a course or track, to careers, to curriculum vitae, and to professional educators, it appears that the word took on its current meaning only late in the nineteenth century.

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WILLARD B. ROBINSON. *American Forts: Architectural Form and Function*. Urbana: University of Illinois Press, for the Amon Carter Museum of Western Art. 1977. Pp. xiii, 229. \$15.00

Architect-historian Willard B. Robinson has written an attractive and orderly architectural-engineering exposition of the evolution of fortification designs as they reflected changing national defense doctrine and planning. The author's thesis is that a clearly defined function produced beauty of form. Thus, because they developed from precise strategic principles, some of America's forts are among the nation's most functional and beautiful pre-twentieth-century structures.

Robinson divides his subject into five chronological periods: (1) development of fortification design from ancient times to Vauban; (2) colonial forts from eastern Canada along the Atlantic and Gulf coasts to the Pacific; (3) transitional forts built in the years between the end of the Seven Years War and 1916, including Revolutionary works and the First and Second Systems designed to protect harbors and important coastal cities; (4) the Permanent System of masonry forts planned "to make the coast, so far as might be practical, impregnable"; (5) land frontier forts, spanning a period that began with Fort Harmar, Ohio (1783) and ended with Fort Assiniboine, Montana (1879). The exposition is supported by 119 illustrations that include copies of maps and plans, sketches, paintings, photographs, and location maps. Especially helpful in understanding fortification terminology are a fine glossary and four schematic diagrams.

Missing are discussions of significant events associated with the forts' histories that would have expanded the reader's understanding of their role as statements of defense doctrine. Missing also are references to weaponry and its interaction with architectural engineering in the evolution of both doctrine and fortification design.

The book is marred by the uneven quality of the research and documentation; and some factual errors result. To cite two: Brigadier John Stanwix was not an engineer and he did not design Fort Stanwix. He commanded at the Oneida Carrying Place when the fort was built (p. 39 and fig. 16). The original plan was laid out by Major Eyres, Captain William Green, and Lieutenant John Williams. Pierre Charles L'Enfant did not design Fort Washington, Maryland (pp. 77, 84, fig. 46); after he failed to produce a plan, he was replaced by Lieutenant Colonel Walker K. Armistead, who submitted the first and basic plan on October 15, 1815.

Robinson has made an important contribution to our understanding of how American forts exemplified changing defense systems and of how the tactical functions of their architectural components shaped their design; both are real services to military and industrial history.

JOHN LUZADER
National Park Service

HERBERT HILL. *Black Labor and the American Legal System*. Volume 1, *Race, Work, and the Law*. Washington, D.C.: Bureau of National Affairs. 1977. Pp. xiv, 455. \$17.50.

In *Black Labor and the American Legal System*, Herbert Hill describes work as "among the most creative of human tasks." From his discussion, however, one might well argue that efforts on the part of white unionists and government officials consisted of a creativity higher than work as they combined to deny blacks access to jobs. This network of collusion, corruption, deceit, and outright violence constitutes a main theme of Hill's book on black labor in the United States.

Black Labor is divided into two parts. Part one deals mainly with the legal aspects of the rights of blacks as workers in the U.S., whereas part two is a history of racial discrimination in employment during World War II as seen through the efforts of the President's Committee on Fair Employment Practice to solve difficulties organized white labor imposed on black workers. Both sections stress Hill's firm belief that black workers stood a far better chance of obtaining support from CIO unions than from those affiliated with the AFL.

The first section of the book contains the most original discussion. Particularly revealing is Hill's analysis of the impact of the Civil Rights Act of 1866 on development of the rights of black people as workers. Hill considers the 1866 act one of the most important during Reconstruction and writes that it has been instrumental in judicial decisions upholding the right of blacks to seek redress against job discrimination. His analysis of this act and of the National Labor Relations Act makes clear his view that courts have proved to be a much better weapon for advancement of the rights of blacks to jobs than have federal administrative agencies. Indeed, it is clear from *Black Labor* that Hill considers the judiciary much less likely than administrative agencies to be in collusion with organized white labor. Hill concludes that it is highly unlikely that the National Labor Relations Board will move to enforce legal prohibitions against job discrimination unless the courts compel it to do so in order to fulfill the constitutional imperative set forth in Title VII of the Civil Rights Act of 1964.

Though in some ways more informative, Part two of *Black Labor* is less original than part one. Part two consists of a series of discussions of developments among major industries during the Second World War and of efforts of the Fair Employment Practice Committee (FEPC) to end racial discrimination on the part of unions, corporations, and government agencies. Hill writes about the shipbuilding industry, aircraft and automobile in-

dustries, public transportation (particularly the Philadelphia case), and, of course, the railroads.

Between its origin in 1941 and its demise in 1946, FEPC investigated, held hearings, and issued directives and orders in all these areas. Yet, by and large, the federal committee was unable to solve the most glaring difficulties black workers faced, even in wartime. The most outstanding failure, according to FEPC itself, was in the railroad industry. And even the semblance of progress occurred—for example the successful desegregation of the work force of the Philadelphia Transit Authority—only after the U.S. Army intervened to maintain transportation as a vital part of the war effort. In the shipbuilding industry, where a modicum of success came on the west coast, the situation bears out Hill's view that court actions achieved the greatest success. Though FEPC expended numerous hours and large sums of money to solve problems black workers faced in the shipbuilding industry, the International Brotherhood of Shipbuilders and the various shipbuilding companies on the west coast moved to improve conditions of employment for black shipbuilders only after those workers sued in the courts of the state of California.

But it is in this section that more could have been said. Hill tells well the stories of the activities of the federal government in these various industries and advances somewhat our understanding of this crucial period. His extensive discussion of the World War II period, however, lacks a clear analysis of why the government considered it in its best interest to maintain a system of discrimination at a time when it was fighting for "democracy" around the world. Hill writes for example that Paul V. McNutt vetoed early FEPC hearings on discrimination in the railroad industry, but he does not tell us why. He even relegates the discussion to a footnote, thus hardly heightening our understanding of this major decision. Moreover, there is little here about the activities of black workers themselves.

In short, *Black Labor* describes and analyzes the circumstances that produced the current employment status of blacks (and, by extension, other racial minorities). Solidly based on a wealth of legal documentation (almost a compendium of court cases on employment law), it is as well a powerful statement in support of those who argue that the nation must take extraordinary steps to redress the grievous errors of the past in employment and economic development. It will prove valuable reading for students, scholars, and those public planners who must help to chart the future of work in the United States.

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CALVIN MARTIN. *Keepers of the Game: Indian-Animal Relationships and the Fur Trade*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xi, 226. \$10.95.

With this brilliant study of the Indian world view as applied to the fur trade, Calvin Martin has challenged the traditional economic explanation of the Indian's abrupt change from a subsistence hunter to an intensively exploitative pursuer of fur-bearing animals. Western bourgeois analysis centering on a marketplace mentality cannot, Martin believes, be either fruitfully or accurately applied to preindustrial societies. In short, it was not just a passion for European goods that would make his life easier and more pleasant that impelled the Indian to become a willing partner in the fur trade. Such a view conceives of the Indian in Western mechanistic and individualistic terms rather than as he really was.

Concentrating on the hunting-gathering tribes of eastern Canada, particularly the Ojibwa and Micmac, Martin delineates the supernaturalistic world view of these Indians and emphasizes the importance of ideology and spiritual factors in the hunter's relationship to the ecosystem. In the Indian's animistic religion, a spiritual power, or *manitou*, pervaded all things, animate and inanimate. The Indian thus looked upon the world about him as being filled not with "things" but with "beings." Humans and animals lived in a contractual relationship, the animals knowing that they were destined to be taken by the hunters to provide for the needs of humans, and the humans knowing that they must not abuse the animal world by being irreverent toward it and killing too many. The animals and their spirits would retaliate against man if man violated this contract by overkilling.

Why then did the Indian's spiritual view break down and open the way to his wanton slaughter of game as he became almost an obsessive participant in the fur trade? The first breach, Martin believes, came as a result of disease and epidemics. Europeans and their animals brought with them new and devastating diseases that Indians did not understand and against which they had no defense. Their holy men, or shamans, were powerless to cure or even curb such disorders as smallpox, measles, tuberculosis, tularemia, and plague. These diseases spread from the coastal regions into the interior, infecting humans and animals even before any whites had been seen in those regions. As a result, Indian confidence was shaken. The world in which the Indian had been so secure, the world that he could control through proper manipulation of *manitou*, was now coming apart. A crisis of confidence resulted. Through examination of

tribal folklore, Martin has concluded that many Indians believed the animals were taking revenge on them. The mutual contract between man and animals was therefore in jeopardy.

Under the pressure of uncontrolled epidemics, missions, and the fur trade, the Indian world view was largely shattered. What had once been in balance was now out of balance. The Indian now entered into the fur trade with an intensity that Martin can describe only as a war of revenge against game.

Martin has not unequivocally disproved that man's method of earning a living, his economy, is fundamental in shaping his ideology, even in pre-industrial societies. The fur-bearing animals, after all, were not being slaughtered basically in retaliation for supposed wrongs. They were being killed in enormous numbers because their furs could be used in exchange for trade items that had become necessities as well as luxuries—items that were profoundly altering the Indians' way of life. The old economic view may yet have some steam left in it.

Martin admits that he is presenting a hypothesis. But it is a hypothesis that is backed up by a breath-taking amount of skillful interdisciplinary research and also one that attempts, with great success, to present the case from the Indian point of view.

BARBARA GRAYMONT
Nyack College

PAUL BOYER and STEPHEN NISSENBAUM, editors. *The Salem Witchcraft Papers: Verbatim Transcripts of the Legal Documents of the Salem Witchcraft Outbreak of 1692*. In three volumes. New York: Da Capo Press. 1977. Pp. ix, 334; ix, 335-699; ix, 701-1070. \$95.00 the set.

Before the appearance of these three volumes, the only substantial printed collection of legal documents relating to the Salem witchcraft trials was *Records of Salem Witchcraft*, printed for W. Elliott Woodward in 1864, which was riddled with errors and radically incomplete. A much more complete and more accurate transcript, compiled by the WPA in 1938, existed only in typescript copies deposited in the Essex County Courthouse in Salem. It is that transcript, with some of its remaining errors silently corrected and with additional materials added, which forms the basis of the present collection.

The additions are of two kinds, the first consisting of documents overlooked by or unknown to the WPA compilers. The most important of these are the death warrant for the five persons executed on July 19, 1692 and seven documents relating to

the case of John Willard. Although there are at least two forgeries of Salem death warrants extant, the only genuine one previously known was that for Bridget Bishop. The Willard documents include an account of the death of Daniel Wilkins, whom Willard was thought to have murdered by witchcraft, and the report of a Jury of Inquest which examined the body and concluded that "he dyed an unnatural death by some cruell hands of witchcraft" (3:822). Also included are two versions of Willard's preliminary examination, in which a central question was whether he could say the Lord's Prayer (popular belief held that witches could not do so because they regularly said it backwards at their Sabbaths). Willard tried five times, and much to his dismay he could not say it correctly. It is now clear that the two central reasons for his execution were the verdict of the Jury of Inquest and his inability to say the Lord's Prayer. These documents, printed here for the first time, throw much light not only on the Willard case but also on George Burroughs's saying the Lord's Prayer to the crowd on the day when he and Willard were executed; he may well have been trying to create an invidious distinction between his case and Willard's.

The second kind of addition is of documents which had previously appeared in print (the WPA compilers worked only from manuscripts). The editors have drawn on many sources, but they have not been as comprehensive here as one might wish. Any compilation of Salem legal documents ought to have included both the old charter law against witchcraft and the English law in effect at the time of the trials (1 *Jacob.* cap. 12), but only the Massachusetts law of December 1692, based on the English law, is present here. There should have been some documentation of the government's maneuverings to ensure that judgments rendered under old charter law and in the interim period should not be held invalid. Also, the order of December 17, 1696 for a fast day in repentance for the trials certainly ought to have been included.

The editors have contributed an introduction, a selected bibliography, a brief glossary of legal terms, and a revised table of contents that they call "the most complete list of the accused witches ever published" (1:36). So it is, except that it is marred by the inclusion of five persons who were accusers rather than accused: Sarah Bibber, Elizabeth Hubbard, Mercy Lewis, Susanna Sheldon, and Abigail Williams. The documents under these names have to do with the unreliability of the accusations they were making rather than with accusations of witchcraft against them.

Whatever the flaws of the present edition, it is very good to have the WPA compilation in print, especially with the added documents in the Wil-

lard case. As the present editors point out, it was not done with the kind of professional care given to recently edited historical papers. But we are not likely to get a better edition; for the foreseeable future this one should be standard. It offers a number of opportunities to historians. With these documents now generally available, perhaps someone will supply the most pressing need in Salem witchcraft scholarship, which is an adequate survey of the various social causes for this most notorious event in our colonial history. To date, social historians have tended to look for a single cause which would offer a comprehensive explanation and, avoiding folk belief and folk behavior, have settled on generational conflict (John Demos) and Salem village factionalism (Boyer and Nissenbaum). Aside from the phrase "old witch," an epithet employed indiscriminately by old accusers as well as young ones, apparently in reference to the stereotype of the witch as an old woman, there is not one display of generational hostility on the part of an accuser to be found in all these three volumes. And as the editors themselves point out (1:17), there are many persons in these documents who had no part in Salem village factional conflicts.

What were the causes, aside from the evidence of some actual practice of witchcraft, aside from the generally precarious state of the commonwealth at this time, and aside from village factionalism, which made so many people suspect their neighbors and even their relations of so terrible a crime? Thorough investigation will probably demonstrate that conjugal hostility was one cause; there are expressions of it in several important cases, including those of George Burroughs, Martha and Giles Corey, Sarah Good, Elizabeth and John Procter, and John Willard. Miscellaneous quarrels between neighbors, unrelated to village factionalism, are another likely possibility. So, very probably, are causes which nobody has perceived as yet.

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MARION BARBER STOWELL. *Early American Almanacs: The Colonial Weekday Bible*. New York: Burt Franklin. 1977. Pp. xviii, 331. Paper \$7.95.

In 1878 Moses Coit Tyler talked of using the almanac to "penetrate to the core of early American literature." Ever since, there has been an obvious need for a full-scale follow-up on the small section of Tyler's *History of American Literature* in which he threw down that intriguing challenge. Marion Barber Stowell's *Early American Almanacs* is by far the most comprehensive effort to deliver so far.

The author carved out an ambitious assignment for herself, choosing no less than to combine a history of the almanac as a genre in colonial America with a literary analysis of the various kinds of writing that the early American almanac contained: epistolary prefaces, essays, "receipts," and so on. The completed assignment appears in two roughly equal parts—the second, or "literary" part, more successful than the first.

The first half of the book, primarily historical and biographical, depends almost exclusively upon the existing secondary literature and therefore adds little or nothing to what we already know about colonial printing in general or almanac-making in particular. Unfortunately, Stowell's research did not disclose nearly as much as we should like to know, for example, about the relative contributions of the "author," printer, and other contributors to the composition of the almanac, about the processes and procedures in its compilation, and specifically about the relative importance of the almanac (in actual pounds, shillings, and pence) to the printer's livelihood. These are some of the aspects of the history of American printing that the classic authors in the field such as Isaiah Thomas and Lawrence C. Wroth did not get to in their respective eras and that Stowell has not gotten to in hers.

The second part of *Early American Almanacs* is far more original and far more useful. For this Stowell's sources were the almanacs themselves—primarily, it appears, the great collection in the library of the American Antiquarian Society. One might wish that she had taken a bit more care in keeping her chronology in line instead of jumping about as she sometimes does, that she had a more definite terminal date for her study, and that she had not played quite so fast and loose with the ubiquitous terms "Puritan" and "Puritanism," by which she apparently means to denote the entire culture of seventeenth-century English America—north, middle, and south.

But Stowell's breakdown of the literary forms in the colonial and Revolutionary American almanac is sensible and helpful, her analysis sure, and her selection of examples a pure delight. Moreover, her use of the almanac as cultural evidence, documenting the obvious shift from religiosity on the one hand and astrology on the other to secularism, rationalism, and science, is done clearly and convincingly.

The book is profusely illustrated with pages and cuts from the almanacs, but it is too bad the reproduction is not better. And one of those illustrations, the one on page 220, confronts the reader with a beguiling question that Stowell does not answer. Why in the world, in *Thomas's Massachusetts, Connecticut, Rhode-Island, New-Hampshire and*

Vermont Almanack for 1789, do we find the following entry for January 30: "K. Charles I. Martyr. 1648"?

CHARLES E. CLARK
University of New Hampshire

JON BUTLER. *Power, Authority, and the Origins of American Denominational Order: The English Churches in the Delaware Valley, 1680-1730*. (Transactions of the American Philosophical Society, volume 68, part 2.) Philadelphia: The Society. 1978. Pp. 85. Paper \$8.00.

After having read so many informative histories of New England, it is refreshing to read Jon Butler's excellent monograph on emerging denominations in the Delaware Valley, a subject more important than many scholars have realized. As Butler points out, not only are there no comprehensive interpretations of religious developments in the valley, but the region was "crucial to the shaping of American denominational life" (p. 6). Three of the most prominent English denominations in America—Friends, Baptists, and Presbyterians—organized there, while another conspicuous group—the Anglicans—was active as well. The religious pluralism and voluntarism of this region made it more similar to what the United States later became than New England and the Tidewater South.

Butler's purpose is to test the "democratic interpretation of the colonial denominational order by examining the English religious groups in the Delaware Valley" (p. 6). He concludes that the stress that historians "have traditionally placed on the rise of democracy and the consequent importance of laymen in colonial religious affairs" is misplaced (p. 75). In the Delaware Valley, clergymen and only a few laymen controlled religious institutions. They learned these "hierarchical patterns" in England, leading Butler to infer that English denominational practices and not the American environment were determinative in the Delaware Valley. Also worthy of note is his insistence that the Church of England failed to flourish there "largely because Anglicans never succeeded in extending their own traditional non-democratic ecclesiastical institutions overseas" (p. 78).

In identifying the implications of this study, Butler claims that it "should help us understand some problems in . . . American religion" (p. 76), including the Great Awakening and nineteenth-century revivalism. He questions whether these movements were as democratic as his colleagues have described them and asserts that the Delaware Valley's most prominent denominations responded to the Great Awakening by maintaining their Old World authoritarian traditions.

Although Butler's conclusions concerning the Delaware Valley in the late seventeenth and early eighteenth centuries flow smoothly from his careful research in the numerous surviving primary sources, he must exercise great care in projecting them to different areas and later times. Despite the importance of the place and time on which he concentrates, judgments based on such limited research do not necessarily invalidate studies of other areas, periods, and movements. His reference to the Great Awakening, for example, is stimulating but too brief and superficial to be convincing (pp. 76-78). If Butler broadens his research and arrives at the same conclusion, scholars will be more likely to accept his suggestion that they reject the democratic interpretation of early American religious history.

JOHN B. FRANTZ
Pennsylvania State University

ROBERT J. DINKIN. *Voting in Provincial America: A Study of Elections in the Thirteen Colonies, 1689-1776*. (Contributions in American History, number 64.) Westport, Conn.: Greenwood Press. 1977. Pp. x, 284. \$16.95.

Robert J. Dinkin's book contains a useful, detailed compilation of our present knowledge concerning the political process before 1776. It covers more ground than the title suggests, for it includes chapters on the candidates and the influences affecting men's choices, as well as qualifications of the voters, campaigns, the number of votes cast, and the balloting. Dinkin stresses the great differences among the colonies but on the whole sees a relatively democratic system rather than an aristocracy. He also argues for an increasing democratization over time, though the reader remains uncertain whether this resulted from the revolutionary ferment or reflected a long-term trend. He introduces a great deal of interesting material. The chapters on nominations, electioneering, and voting procedures, in particular, create a fascinating picture of intense activity, with more popular participation than we usually suppose—though gathering all this together may distort the actual situation. Students will find the bibliography useful, while lamenting the position of footnotes at the rear of the book.

Dinkin leaves unresolved a major question about the franchise. Colonial restrictions relating to age and property really meant that the young man could vote only when he became a stable, responsible member of the community. Usually this occurred when he married, combining his own inheritance with that of his wife to achieve an independent income. If we accept the colonial view instead of our own ideas of the right to vote, we

should inquire how many men could look forward to full participation in politics when they reached maturity. At present we do not know. Estate inventories and tax lists contain the necessary information when combined with the ages of the persons involved. The same arduous research in unpublished sources is needed for office holders. Someone will have to rewrite these sections entirely, for at present they rely too much on guesswork.

Unfortunately, the style of the book is labored and dull. The subject has so much intrinsic interest, and Dinkin draws together so much scattered information, that one would like to recommend it heartily for outside reading in undergraduate courses, especially if it appears in paperback form. But it is heavy going and too often unclear. The sentence "If closed balloting proved adequate in some colonies, those without it rarely demanded its adoption" (p. 139) would not have survived a good editor, and the publishers did the author, themselves, and readers a disservice by failing to provide one. The book remains a valuable reference for historians of political and early American history.

JACKSON T. MAIN
State University of New York,
Stony Brook

SALLY SMITH BOOTH. *Seeds of Anger: Revolts in America, 1607-1771*. New York: Hastings House. 1977. Pp. xiii, 301. \$9.95.

Sally Smith Booth's *Seeds of Anger* recounts the major violent protests and rebellions that disturbed seventeenth-century British North America and adds a few from the eighteenth century for good measure. She briefly acquaints the reader with unrest in early Virginia; friction between Maryland's settlers and Lord Baltimore; conflicts between Catholics, Anglicans, and dissenters; Anglo-Dutch rivalries; and uprisings against the Carolina proprietors. Bacon's, Coode's, and Leisler's rebellions in Virginia, Maryland, and New York and the overthrow of Governor Andros in Massachusetts receive more extended treatment. Dramatic narratives of America's slave revolts, the march of the Paxton Boys in Pennsylvania, and the Regulator uprisings in the Carolinas conclude the work.

Some of these episodes are presented effectively, particularly Bacon's Rebellion, the fall of the Dominion of New England, and North Carolina's Regulator movement, although each lacks a balanced analytical framework. Booth's discussion of South Carolina's Regulator movement is stronger, and her best chapter, on black rebellions, is both well researched and powerfully written. Occasion-

ally rewarding sections, however, cannot begin to rescue this book from its difficulties. The prose, poorly edited and cast in choppy paragraphs entirely lacking in appropriate transitions, generally oscillates between stark narrative and unsubstantiated (and occasionally irrelevant) generalizations. The research is often inadequate and out of date; much of the innovative work of the last decade has been ignored, most notably Edmund Morgan's brilliant recreation of life in early Virginia.

Perhaps Booth has ignored such works because their common objective, a historical understanding of the roots and consequences of conflict in early America, does not greatly concern her; for what *Seeds of Anger* ultimately lacks is any comprehensive understanding of the behavior of colonial Americans. Just how violent were the colonists, in the context of the seventeenth- and eighteenth-century British world? Did rebellious behavior increase or decrease over time? In what ways were colonial societies divided into mutually hostile groups? Exactly what features of British colonial administration angered Americans and why? Booth gives no coherent answers to these questions, but in the course of her narrative she does suggest that America's pioneers were more independent and less obedient to all governmental authority than their countrymen in Britain, that the level of colonial violence was fairly constant, that the important social divisions were those between religious groups and between rulers and "the people," and that British rule was nearly always resented, by nearly every uncorrupted colonist, because it was both foreign and elitist. Each of these sweeping generalizations deserves, and has often received, serious study. None serve well as a foundation for an intelligent narrative of early American rebellions.

It seems unlikely that any scholar will find this book useful. Since the work was probably intended for general readers, however, one may ask what profit they might derive from it. For those who want a short account of America's black rebellions and back country uprisings and can only spare an hour or two, the last three chapters are serviceable. The first three-quarters of the work, however, will only confuse the general reader, without providing compensating entertainment. Far better alternatives exist, both for pleasure and for instruction.

RICHARD ALAN RYERSON
Harvard University

PETER SHAW. *The Character of John Adams*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg. 1976. Pp. ix, 324. \$12.95.

Applying the techniques and insight of literary analysis, Peter Shaw has written an "inside biography" solidly based on an impressive mastery of myriad sources. Chief among these are the Adams Papers housed at the Massachusetts Historical Society, but Shaw has also carefully combed both the manuscript and published records of Adams's contemporaries. He has also utilized a tremendous array of secondary works, most notably the results of modern scholarship that have all but revolutionized the historiography of the Revolution.

Shaw suggests that rather than "touching on all the events of Adams's life," he will "view his character, thought, and acts as a whole" (p. viii). The work, nevertheless, is organized chronologically, not topically. It is a life of Adams, though at each stage of Adams's career Shaw succeeds brilliantly in synthesizing "character, thought, and acts." "If I have a formula for reconciling the public and private Adams," Shaw writes, "it has been to intellectualize his behaviour and to personalize his ideas" (p. viii). Shaw consistently holds to this methodology; it is a formula precisely suited to elucidate the life and character of this enigmatic man.

Shaw's Adams is a complex concatenation of contradictory qualities. To turn upon Adams his own favorite technique of "stringing together epithets," he was contentious, cranky, wrong-headed, impolitic, headstrong, intense, independent (above all); he had an instinct for unpopularity, was studious, feared giving vent to his feelings, and reacted to aggression with mildness and friendship. The "key" to Adams's character is self-denial; to his philosophy of life, emulation.

Like his subject, Shaw writes with economy and clarity in a dense prose style that disdains the flourishes of rhetoric. We learn that Deacon John Adams and James Otis were Adams's models for emulation; Thomas Hutchinson, Franklin, and Hamilton his opponents. Of these last, Franklin fares the worst not only because Shaw likes Adams and Adams disliked Franklin, but also because Shaw relies upon Cecil Currey's *Code Number 72. Ben Franklin: Patriot or Spy?* (1972), a work concerning which the verdict is not yet in.

Neither is the verdict in on psychohistory. Similar to Fawn Brody's Jefferson, Shaw's Adams now and then misdates a letter or uses a "code word" as in "mulatto" for Jefferson and "piddling" for Adams. The implications of these acts and words—at least to the authors—are quite earth-shaking. One must be fairly warned: misdate a letter at your peril. A future biographer may discover from your error that you were "on a second honeymoon" (p. 195) or suffered mentally last February (p. 65). If perchance you use the word "murder" to describe the felling of a walnut tree,

you were reliving the politics of a quarter century past (p. 245). Adams's efforts to clothe the presidency in pomp and titles, we are told, were unconsciously motivated in the need to make amends to George III against whom he had "flirted with parricide" (p. 229n).

To such claims, one at best might say, "maybe." But to most of this book one must say a resounding "yes." Shaw composes the finest capsule analyses of Adams's political works now available. His contrasting of Franklin's and Adams's autobiographies is itself worth the price of admission: "Franklin's story reads like a lesson in what Adams should have done, Adams's as a cautionary tale of what might have become of Franklin" (pp. 279-80). Marred only by the inroads of questionable psychological guesswork and a very few errors (Shaw does not mean the oft-misused word "fulsome" on p. 181; Jefferson replaced Franklin, not Jay [p. 195, stated correctly, p. 202]), Shaw's *Adams* is a model of the biographer's art. "Sobrius esto John," Adams warned himself against getting too excited over his books. Concerning this book, sobrius esto reader. It will stimulate the glands of anyone interested in Adams and the origins of the Republic.

THOMAS WENDEL
San Jose State University

PAUL H. SMITH, editor. *Letters of Delegates to Congress, 1774-1789*. Volume 1, August 1774-August 1775; volume 2, September-December 1775. Washington: Library of Congress. 1976; 1977. Pp. xxxvii, 751; xxvii, 585. \$8.50; \$9.00.

Among the several projects initiated by the Library of Congress to commemorate the bicentennial of the American Revolution, this definitive edition of the "writings" of the delegates to the Continental Congress may well prove to be the most lasting. These first two volumes carry the documentary record forward through December 31, 1775. Brought together for the first time in this edition are fifty-seven letters written between the dissolution of the first Congress in October 1774, and the convening of the second in May 1775, furthering our knowledge of the delegates' activities and immediate concerns during these critical winter months. Because the editors have confined these volumes to the years 1774 and 1775, they should be able to collect material relating to the question of independence in volumes three and four. It suggests they have given some serious thought to the compartmentalization of these writings and have adopted a refreshingly practical approach.

In a carefully crafted introductory essay, "The

Continental Congress, 1774–1775,” the editors note that the period was one of “profound importance to the people of the 13 colonies who sent delegates to Philadelphia. It was during these months that they began to acquire an identity that distinguished them from their fellow citizens in other parts of the empire. . . .” The editors insist, however, that “there is little in these pages to indicate that the delegates were prepared to renounce their status as subjects of the crown and citizens of the empire.” If anything, they hoped that the “justness of the American claims” would be conceded and the “delicate and complex equilibrium of empire” restored (1:xix). Collectively the documents reinforce the proposition that most of the delegates focused on practical issues and had an essentially colonial frame of reference. The period is thus defined in its own terms rather than cast in the idiom of 1776.

Inevitably these volumes invite comparison with Edmund C. Burnett’s eight volumes, *Letters of Members of the Continental Congress* (1921–1938). Because his volumes have achieved such a revered place in American historiography, the initial plan was to “supplement” his work. When the editorial staff realized how much additional material was available, they decided to prepare a comprehensive new edition which would replace Burnett. As one reads between the lines of Elizabeth Hamer Kegan’s “Foreword” and the editors’ “General View of the Work,” one senses that they all expected to be drummed out of the profession for even thinking of a new edition.

But these volumes need not have been introduced with such trepidation, for this is a better edition of “writings.” The documents are printed in their entirety; there are more of them; the notes accompanying the documents are fuller, yet not pedantic; the index is superb; and the illustrations have been selected with a sense of purpose. There is, of course, ample opportunity for the specialist to quibble with this or that and argue that Burnett’s way was better, but most historians of the period will be hard-pressed to find something worth challenging. Rather, the editors have kept themselves out of the limelight, quietly employing a level of editorial craftsmanship that assists rather than engages the scholar. In truth, they have embarked upon a project that in form and substance must elicit our respect.

Despite the superiority of these volumes, Edmund C. Burnett’s scholarship continues to dominate the historiography of the period. It was his *Letters* that proved to us that the writings of the delegates were indispensable to an understanding of the Congress. And with his *Letters* and *The Continental Congress* he made the period come alive, vividly broadening the cast of characters in the drama

known as the American Revolution. It is his perceptions and his sensitivity to the human dimension in history that validate this project.

JOHN J. REARDON
Loyola University,
Chicago

RONALD M. PETERS, JR. *The Massachusetts Constitution of 1780: A Social Compact*. Amherst: University of Massachusetts Press. 1978. Pp. xiii, 242. \$15.00.

What was the “most important question that faced the framers of the 1780 Massachusetts Constitution”? “How can we give to the virtuous people a sufficient amount of control over their corruptible rulers?” Ronald M. Peters, Jr., asks and answers, in this workmanlike and readable effort to make John Adams’s handiwork rank alongside the Declaration of Independence, federal Constitution, Bill of Rights, and the *Federalist* “as one of the five most important documents of the revolutionary era” (p. 13).

Does Peters pull it off, to the point that we will need to rewrite the syllabus on constitutional law and give due credit? No, but he has written a treatise on political theory almost free of jargon and based on a thorough gleaning of contemporary newspapers and pamphlets, for which he deserves much credit. Despite a number of pedantic slips (there are many “it has been learned,” “let us proceed to consider,” and similar phrases straight out of *Political Science* 101), Peters shakes free of the theoretical shackles he imposed at the outset and by his third chapter has developed his theme with insight and skill.

He claims too much, of course, but that is the way with many good dissertations. Not everyone can be a Leonard W. Levy, however, and get away with calculated risks, although Peters comes close, owing to his array of evidence taken from contemporary essays, sermons, and letters. His explanation of how the town meetings deliberated such weighty questions as “what is civil liberty?” and “what is religious freedom?” shows mature judgment. Peters also recalls that the idea of a written constitution sprang from the English experience of the seventeenth century, and had to be based on majority rule. The efficacy of the majority, as Peters spells it out, almost reads like a tract for Jackson’s 1828 campaign. But even the majority needed some kind of check, and the Massachusetts convention used “the principle of separation of powers as one means of effecting a limitation on the power of ephemeral majorities” (p. 176).

Although Peters makes his point about a utilitarian social compact theory, he has some trouble giving credit to the experience Americans had ac-

cumulated in self-government prior to 1775. He tends to gloss over the fact that John Adams wrote most of the constitution by himself, and talks a good deal about "the framers" as though intensive debate preceded Adams's *tour de force*. Even worse, Peters apparently never heard of George Mason and chooses to ignore the Virginia Declaration of Rights of 1776—a serious and unexplainable omission since Adams borrowed from Mason right and left.

Still, on balance Peters has done well to remind us that at the outset our constitution-makers were "in pursuit of the public good" and placed that goal above "any concept of antecedent individual rights or prerogatives" (p. 194). There is the ring of truth in these assertions nearly two hundred years later.

ROBERT A. RUTLAND
University of Virginia

LANCE BANNING. *The Jeffersonian Persuasion: Evolution of a Party Ideology*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 307. \$15.00.

Lance Banning's major thesis is that from 1763 until at least 1815 every major American political issue was tied to the question of how liberty could be preserved in a popular government. As Bernard Bailyn, Gordon Wood, and more recently Rodger Parker and Forrest McDonald have done, he finds Americans repeatedly turning back to the opposition rhetoric of seventeenth- and eighteenth-century England.

Banning shows with convincing thoroughness that the first American party system cannot be understood without examining how the English opposition tradition prepared both Republicans and Federalists to see the 1790s as another—perhaps the final—crisis in the troubled history of liberty.

He follows opposition thought in England to the mid-eighteenth century, stressing the shift from "mixed" government to "balanced" government which made opposition ideology acceptable to American republicans. Banning emphasizes also the opposition tendency to see executive conspiracies as the greatest threat to liberty, manifesting themselves in undue "executive influence" over the legislatures, a standing army, and a "paper aristocracy" based on banking and the public debt.

This strain of thought, which pervaded America before and during the Revolution, was temporarily dethroned in the 1780s by a contrary but still classical view which saw liberty in danger from its own excesses, especially from overpowerful and fanatically provincial state legislatures. The ratification struggle of 1788–1789 was an open conflict between these two views.

Banning finds American politics during the 1790s dividing more and more clearly along this fissure. His most interesting conclusion is that Federalists viewed politics from the same underlying assumptions as Republicans did, springing from the same roots and agreeing on all the fundamentals of republican philosophy. Both parties wanted a balanced government but thought such a balance was terribly fragile; both believed the natural tendency of republics was to decay into tyranny; and both were desperately anxious to preserve liberty from the dangerous policies of their opponents. The parties disagreed simply over where the greatest danger to liberty lay. Men who became Republicans followed the English opposition in fearing both executive encroachment on legislative independence and erosion of the middle-class social order on which republicanism depended. Federalists, remembering the 1780s, still thought the real threat to liberty came from irresponsible demagogues in Congress and the state legislatures.

In this great debate Republicans had two decisive advantages. First, their perception of the threat to liberty was much closer to that which all Americans had shared after 1763 and upon which the legitimacy of the Revolution was grounded. Of more importance, the details of the Federalist program after 1790 reproduced almost perfectly the very evils which opposition ideology pointed out as most dangerous to liberty.

In drawing these conclusions, Banning relies chiefly on public rhetoric—newspaper articles, pamphlets, and speeches. He makes a good case, however, that this ideology was shared by Republicans generally and not confined to the few men he actually studies. Footnotes relate his arguments to earlier studies of the period and record his firm but courteous disagreement with some other scholars. Altogether, he has produced an intellectual analysis which will be fundamental to future work on the 1790s.

JAMES H. BROUSSARD
*Centennial History of the Indiana General Assembly,
Indianapolis*

CHARLES T. CULLEN and HERBERT A. JOHNSON, editors. *The Papers of John Marshall*. Volume 2, *Correspondence and Papers, July 1788–December 1795; Account Book, July 1788–December 1795*. Chapel Hill: University of North Carolina Press, in association with the Institute of Early American History and Culture, Williamsburg. 1977. Pp. xxxvi, 547. \$18.95.

This carefully and intelligently edited volume of John Marshall's writings brings together valuable

information about important aspects of the famous Chief Justice's pre-judicial career.

Much of the material deals with Marshall's emergence as a successful and leading member of the Virginia legal establishment. Included, therefore, are a great number of documents that shed light on the everyday activities of a practicing attorney in the early national period. There are assignments, certifications, deeds of trust, petitions, notices, opinions, leases, bonds, affidavits, declarations, stipulations, and various other miscellaneous law papers. Also included are a number of Marshall's arguments in particular cases culled from extracts in the Tucker-Coleman Papers at the College of William and Mary, as well as the eight extant opinions from the six-month period that Marshall served as Acting Attorney General of the Old Dominion. Of special value are the documents on Marshall's role in three of the most important Virginia cases of the period: *Bracken v. College of William and Mary*, the Fairfax land disputes, and *Commonwealth v. Randolph*. Each is introduced by editorial essays that are models of their kind: brief, informative, and without excessive interpretation.

The picture of Marshall that emerges is one of a man on the make, determined to secure himself financially. There is also some material about his activities as a Freemason. More importantly, there is documentation, introduced by a valuable editorial essay, about his role as a brigadier general in the state militia. During these years Marshall also emerged as an important local spokesman for the Federalist party. His "Aristides" and "Gracchus" essays, in response to James Monroe's "Agricola" articles, are clever pieces that support George Washington's neutrality proclamation, criticize Edmund Genêt's activities, and defend the administration from Republican charges of being hostile to France.

The final part of the volume reprints Marshall's personal account book for the years 1788-1795. It reveals much about both his business and private activities and should be valuable to social historians, for it tells a great deal about everyday life in Richmond during these years.

This volume is a scrupulous piece of work bringing together unusually disparate material from recalcitrant sources on one of the most important Founding Fathers. There are three different indexes: one to cases listed in the account book, another to the volume's legal materials, and a general index. It is a work of high quality, which is what we have come to expect from books published by the Institute of Early American History and Culture and the University of North Carolina Press.

RICHARD E. ELLIS
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College at Buffalo

G. S. ROWE. *Thomas McKean: The Shaping of an American Republicanism*. Boulder: Colorado Associated University Press. 1978. Pp. xiv, 503. \$15.00.

Two years younger than George Washington, Thomas McKean retired as governor of Pennsylvania in the final months of Thomas Jefferson's administration and made his last appearance in a public post during the War of 1812. Few Americans, if any, have held so many major offices, and mention of a few of his important positions is sufficient to suggest his service to the revolutionary era that his life so neatly spanned. He was one of a triumvirate who moved reluctant Delaware toward independence, a member of the Stamp Act Congress, and an ally of the Adamses and Lees in the great intercolonial gatherings that followed. He signed the Declaration of Independence and played a special part in bringing it about by acting simultaneously as congressman from Delaware and chairman of Philadelphia's revolutionary committees. A delegate from Delaware to Congress from 1774 to 1783, as well as member of that state's assembly (1762-79) and its constitutional convention (1776), he was also (for a while, concurrently) the first chief justice of the Commonwealth of Pennsylvania (1777-99) and a frequent speaker in Pennsylvania's conventions to ratify the Federal Constitution and reform its own. His life threads in and out of nearly every theme that interests current students of the Revolution, and his twists and turns present a fascinating challenge. A radical, Scots-Irish Whig who early pressed for independence, he opposed the democratic Pennsylvania constitution, then took a crucial post at the behest of its supporters, and yet played no small part in its revision. A Federalist of 1787, he served three terms as Jeffersonian governor, but became increasingly disillusioned with the party's course as the War of 1812 approached.

This is the second recent study of McKean. For all but details of the private life, it supersedes John Coleman's 1975 biography, which broke off without explanation in 1780 when the subject was still in mid-career. Seventy pages of endnotes testify to the meticulous research. A fine first chapter introduces the early influences that would persistently guide the adult. G. S. Rowe sees McKean as a youth who had imbibed the powerful ambitions and contentiousness of his paternal line, accepted the values of his Presbyterian atmosphere and teacher, but prospered through his connections with his more genteel maternal relatives and turned his back on the rougher, more humble dimensions of his "frontier" upbringing. The Presbyterian morality, the Anglophobia (despite his lifelong effort to Anglicize the law), and the conjunction of intense ambition with desire to serve go far to help explain this rising man. McKean's ca-

reer, in turn, becomes a focus for some excellent material on the establishment of Pennsylvania's courts.

Regrettably, however, Rowe is not successful in his fundamental purpose "to explore McKean's impact on American republican ideals and practices" and their impact on him (p. xiii). McKean would seem a perfect subject, but the record is apparently too sparse to trace the evolution of his thought. Occasionally, the evidence becomes more adequate, but Rowe's presentations of McKean's ideas seldom raise the most interesting questions. Nor does the author usually provide clear guidance through the maze of local politics. Only in the case of Pennsylvania between 1802 and 1805 is there an adequate description of the local structure and McKean's position in it. Seldom do we get a concrete understanding of McKean's own stance and goals. The biography has much to recommend it, but there is also much to frustrate readers seeking broader understanding of the revolutionary process.

LANCE BANNING
University of Kentucky

JACOB E. COOKE. *Tench Coxe and the Early Republic*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg. 1978. Pp. xiv, 573. \$25.00.

This is a detailed study of the life and writings of an exceptionally talented man who was also one of the most disagreeable figures of the young nation's public life. An independent and strong intelligence, an energetic aide to others, and a voluminous and influential essayist, Tench Coxe left a lasting mark upon early republican government and policy. But he was also a man of notoriously defective character. Skilled in making enemies and a master of invective, Coxe was a far from stupid man. *Driven by private demons, he was just a fool.*

The surviving Coxe archives are so large that an author might have mistaken volume for a measure of importance. Jacob E. Cooke has avoided this snare and has not tried to cover everything. Instead, he has written a measured, critical assessment of Coxe's contributions to public affairs. What we learn contributes, sometimes significantly, to our knowledge of the period—although no major reinterpretations of events will be necessitated. The text, though compressed by the author's self-denial, is long and demanding. Nevertheless, this is probably the best biography of a second-ranking public figure of the new nation to appear in a decade.

The son of a wealthy Philadelphia merchant, Tench Coxe was by turns a Tory, merchant, publi-

cist, political economist, public official, candidate for elective office, and land speculator. He made his greatest contribution to national affairs with his pen and turned out a staggering mass of writings from the 1780s to the 1820s. By identifying many of these writings and providing qualified attributions for many more, Cooke has made possible a more accurate knowledge of the newspaper and pamphlet literature of the time than we now possess. More important, he has analyzed Coxe's contributions to many of the principal state papers of the day and to the government's early administrative practices. Coxe's ideas on banking, Cooke argues persuasively, influenced Hamilton's "Report on a National Bank," and Coxe decisively affected the "Report on Manufactures." His research and writings also won the respect of Jefferson, who embodied much of their contents in his own official reports. Cooke makes a convincing argument that the range and learning of Coxe's writings entitle him to a rank among the major political economists of the day.

This otherwise thorough, judicious, and indispensable book leaves one puzzled about the significance of Coxe's career, especially as regards the many issues now being addressed by historians of the period. Take, for instance, the problem of the era's unrestrained rhetoric. Most recent historians, myself included, have stressed the origins of this rhetorical venom in republican ideology; Cooke suggests by implication that heated expression was also rooted in the uncertainties of roles and careers. In an open, commercial society, especially a postcolonial one, blatant careerism can occasionally succeed. Thwarted, it may give way more readily to bitterness than in more mature societies, because opportunities, apparently so great and numerous, are in fact limited. If there is any merit to such a view, we may have to revise our hypothesis that venomous language stemmed principally from republican anxieties. It may have had less to do with ideology than with sociology.

Similarly with political convictions: fortunately, we have taken them seriously in recent years. But Coxe's multifaceted public career is a reminder that political beliefs and relationships—and especially political apostasy—can be decidedly personal and not ideological. As Cooke points out, Coxe became an adherent of Jefferson with Hamiltonian views. It is probable that he would have remained a Federalist had he not given Hamilton and John Adams every reason to repudiate him. Scorned and ostracized by his first party, he then sought preferment among the Republicans—and to some extent found opportunities, though few jobs. Through all these vicissitudes, most self-induced, Coxe retained his early convictions and sought to use them in others' service. Had he been

as stable personally as he was ideologically, he might have been a greater figure still.

JAMES M. BANNER, JR.
Princeton University

C. WILLIAM HILL, JR. *The Political Theory of John Taylor of Caroline*. Cranbury, N. J.: Fairleigh Dickinson University Press. 1977. Pp. 343. \$18.50.

John Taylor (1753-1824), remembered as a Jeffersonian fundamentalist, has long been considered a major political writer. C. William Hill, Jr. concurs and argues that Taylor was no mere ideologist but a political theorist (p. 17). Taylor's work must, therefore, be understood to comprise "a comprehensive system of thought . . . [covering] a wide range of topics in an integrated fashion" (p. 282). Thus, Taylor's chief doctrine was not states' rights but the division of power from which his defense of states, and most all of his system, derived. Nor was Taylor the prophet of secession William Dodd held him to be. No one deplored Southern exceptionalism and sectionalism more than Taylor.

These revisions are salutary. They are the result of Hill's research and especially of his exegetical method that takes him beyond the wearisome clichés of earlier, typically historicist studies of political writers such as Taylor. But Hill's challenge to the "history-of-ideas approach" in favor of studying "Taylor's ideas for their own value" (p. 284) does not go half far enough. A behaviorist in the tradition of B. F. Wright and Alfred Cobban, Hill regards theory as a second-best reality. Although he mentions that Taylor's theory is a form of political hedonism (thus invoking Leo Strauss's seminal critique of Hobbes and the contract theory), Hill's own theoretical grasp—hence his analytical reach—is too limited to make use of Strauss.

In fact, Hill makes no effort to employ the newer, more ambitious behaviorisms in use among Marxian critical theorists and fashionable non-Marxian practitioners for whom theory is a reciprocating function of language and material factors. Accordingly, Hill's praiseworthy intention of judging Taylor's theory for its "intrinsically important ideas" comes to little more than rude pragmatism. Hill's opinion that truth is the "composite of divergent points of view" is something less than adequate to his purposes. But of course a theory's value in practice has no logical connection to its intrinsic importance since applications are circumstantial, that is, extrinsic. If applications were to be considered proper tests of intrinsic importance, we would be forced to suppose all circumstances are alike. This absurdity suggests the matter of intrinsic importance is not circumstantial and that making it so will result in presentism. This is just

what happens in this study. Hill's understanding of theory prompts him to look upon Taylor's work from the standpoint of present interest in "equality"—itself not a theoretical term but an element in certain modern movements.

Hill's claim that Taylor's "doctrine of actual human equality was his most original idea and fundamental to his understanding of his stages of aristocracy" (p. 314) seems plainly a misreading. Taylor did not believe that "substantial widespread equality in abilities was already the case" (p. 267). Neither did he think some ultimate mental leveling would bring about the end of "aristocracy," a term Hill equates with "elites" (p. 98). But Taylor did not oppose elites in Hill's sense, which is why the Virginian extolled the county court system in spite of the fact that Hill sees this as a violation of "nearly every aspect" of his theory (p. 211).

Using Hill's own enumeration of Taylor's supposed theory—his individualism, his geographical determinism, his vision of states as "distinct" nations, and most important, using Hill's reading of Taylor's serpentine, and probably incoherent, view of representation—it is quite impossible to sustain the author's attribution of "actual human equality" to Taylor. After all, Taylor's great complaint about John Adams, in response to whom he wrote his major work, was that Adams and all others who conceived government as a balancing of social orders, imposed upon society "permanent and equal distribution of power and mental capacity." In sum, this book, which makes several valuable contributions to the elucidation of Taylor's thought, falls rather short of establishing, or developing, Taylor's reputation as a theorist.

ROBERT J. LOEWENBERG
Arizona State University

NATHAN REINGOLD, editor. *The Papers of Joseph Henry*. Volume 2, November 1832-December 1835, *The Princeton Years*. Washington, D.C.: Smithsonian Institution Press. 1975. Pp. xxxix, 524. \$30.00.

Joseph Henry (1797-1878) was a mostly self-educated American scientist who made important discoveries in the fields of electricity and magnetism and who was also the founding secretary of the Smithsonian Institution. This volume contains primarily letters and laboratory notes from the first three years that Henry served as professor of natural philosophy at Princeton, just after he made his mark in physics as a competitor with Michael Faraday. The papers concern largely the Henry family's attempts to settle into Princeton, Henry's work in resuming his research, and his growing acquaintance with the scientific community in

Philadelphia and, to a lesser extent, elsewhere in the United States.

This is only a selection of the papers that have been sought out with zeal and shrewdness; a microfilm edition will cover the entire collection. But even for the selected papers, fifteen volumes are projected—of which this is volume two. The physical makeup is extravagant, the annotations at least as extensive as the textual material. The question has to be raised as to what the purpose of the publication is.

The book is, for one thing, a monument. It also is the token representative for American science among the many editions of multi-volumed papers of great men. Finally, these volumes serve to permit the senior editor, Nathan Reingold, to write selectively about nineteenth-century American science. (He previously published a documentary history containing important conceptual innovations conveyed through organization and headnotes without references; at least in the present volumes notes contain citations to assist colleagues.) In the introductions and footnotes, then, are not only a partial biography of Henry but numerous little monographs on science in those years. The most important in this volume concern the scientific community, including the Philadelphia "Wistar parties" and "the Club," which latter preceded the notorious Lazzaroni elite of the mid-century. The discussions of Henry's priorities are illuminating, even though muted with hints of apology for an American's disadvantages. Unfortunately, however, the bulk of the annotations are antiquarian in tendency, identifying people or items that are obvious or that no one would need to know about except in the most bizarre circumstances. These annotations are, moreover, often repetitious or inconsistent.

In short, the audience for this book cannot be someone who would just read through, nor can it be the research scholar, who would need the full original sources. It is a pity that in the present hard times the money and talent devoted to these often trivial documents are not freed to produce history more directly and more profitably. Other, similar projects concerning political and military figures have also been questioned in recent years. Perhaps the sponsors of the projected and probably inevitable Alexander Graham Bell papers can emulate the strengths of the Henry papers but avoid compulsiveness and expense.

Finally, it is worthy of remark that in their extensive discussions of American science in the Jacksonian era the editors manage the feat of not once citing George H. Daniels's standard monograph on that subject.

JOHN C. BURNHAM
Ohio State University

EDWARD PESSEN. *Jacksonian America: Society, Personality, and Politics*. Rev. ed. Homewood, Ill.: Dorsey Press. 1978. Pp. xvi, 379. Paper \$7.95.

In the original edition of *Jacksonian America* (1969), Edward Pessen depicted a society that was "less than egalitarian." Now, in this substantially revised and expanded version, he pronounces the judgment that the Age of Jackson was "an age of inequality, whether in material condition, status, opportunity, or influence and power" (p. 100). On the face of it, this may not seem a remarkable conclusion, but in the terms in which it is set forth by Pessen, it is a provocative thesis.

Incorporating relevant scholarship of the past decade, including his own, this edition strengthens and amplifies Pessen's distinctively negative appraisal of Jacksonian America. Five chapters (one, two, seven, eight, nine), remain virtually unchanged. In chapter six, discussions of the new economic history and the profitability of slavery have been cut, the account of agriculture has been compressed, and the segment on slavery has been expanded. The original chapter on "Social Developments" is extended to two chapters, which present an even grimmer view of the plight of the common people, add material on blacks, and include new sections on education, the press, and medicine.

Four chapters on politics (ten through thirteen) are close to the earlier format, but they are enriched in detail by references to dozens of recent studies. Pessen modifies his previous contention that there were no marked differences between Whigs and Democrats, conceding that they presented alternatives to the voters, but he sees this as merely an electoral ploy. Both parties were under conservative control; their leaders were seeking pelf and power. The Jacksonians are now charged with being not only proslavery, but also "anti-black" (p. 301). New evidence suggests that wealthy men were fairly evenly distributed between the two parties, but not much significance can be attached to this finding inasmuch as neither party consulted the interests of the common man. Pessen examines the ethnocultural interpretation of voting behavior and finds it unconvincing because of its unfounded assumptions and weak evidence.

His central thesis is set forth in chapter five, "The Inegalitarian Society." Here he addresses the question of whether Tocqueville's "egalitarian theory of equality of condition" (p. 77) offers an accurate interpretation of Jacksonian society. The answer, based on "startling" (p. 81) new evidence, is "no." Jacksonian America was rigidly divided into two classes, the opulent rich and the miserable poor. There was negligible mobility, upward

or downward. Offices at all levels were held by the "inordinately well-to-do" (p. 97), and politics provided no leverage for common men, who were victims of demagogues and their own gullibility.

Pessen stands both Tocqueville and Arthur Schlesinger, jr., on their heads with his projection of a class-ridden society in which politics ignored the common man. He does not seem to like anything about the Age of Jackson, least of all the Old Hero. This is strong stuff. Despite its assets—vast erudition, bold prose, and excellent bibliography—*Jacksonian America* is open to criticism because it reads like a general indictment. Did Pessen ask the right questions? Were the ills of Jacksonian society peculiar to that age? Was the class structure that simple and rigid? Were there no redeeming virtues?

RICHARD P. MCCORMICK
Rutgers University,
New Brunswick

CHARLES. M. WILTSE, editor. *The Papers of Daniel Webster: Correspondence*. Volume 3, 1830–1834. Hanover, N.H.: University Press of New England, for Dartmouth College. 1977. Pp. xxvi, 547. \$22.50.

Anyone who has worked through the voluminous collections of antebellum political leaders will recognize at once that this third volume of Daniel Webster's *Correspondence* is a masterpiece of selection and compression. Deftly composed headnotes introduce the reader to each grouping of letters, many of which are published here for the first time. The headnotes offer succinct interpretations of the choices Webster faced and link those choices to the emerging political divisions of the 1830s. The result of the editors' skill is a volume which includes some of the most revealing letters of the period and an editorial narration which illuminates the formative years of the second party system.

Until 1830, Daniel Webster was an important member of the supporting cast of national political life. In the years encompassed by the third volume of *The Papers of Daniel Webster*, 1830 to 1834, the Massachusetts Senator moved to the very center of the stage. Issues emerged in this period which would dominate political discourse until 1844—nullification, the Bank of the United States, the tariff debate, and the power of the Executive. Partisan of the tariff and bard of the Bank, early opponent of nullification and belated critic of "executive despotism," Webster often wound up on the losing side of these disputes. But he was eloquent in opposition and by 1833 openly sought the presidency. The *Correspondence* allows the reader to observe the interplay of the unfolding issues with

Webster's political aspirations and to witness the first flowering of the New Englander as a presidential candidate.

Appropriately, the volume begins with the January 1830 debate between Webster and Robert Hayne of South Carolina over the doctrine of nullification; it was his defense of national supremacy that firmly established Webster's position as a national leader. Webster's stand in the Hayne debate elicited an immediate outpouring of adulation. In the Senate, the galleries, and the capitol, he wrote in February, 1830, "the feeling of this little public is all on our side. . . . I never before spoke in the hearing of an audience so excited, so eager, and so sympathetic." The ensuing flood of praise from around the country suggested that Webster had, in the minds of many, transcended the role of New England spokesman to become the voice of the Constitution and the Union.

Party divisions and presidential aspirations were inextricably entwined with the nullification issue, for Webster and for others. The relationship between Webster, his party comrades, and his ally Henry Clay, was badly strained by the real nullification crisis, which occurred in 1833 when South Carolina translated theory into fact by formally nullifying the Tariff Act of 1832. Henry Clay and John C. Calhoun devised a compromise tariff reduction to avert conflict, and Webster was furious with his party colleague at this apparent sacrifice of principle. The estrangement deepened when Clay won important New England support for the compromise. A more important revelation of the *Correspondence* emerges from letters showing how Webster and antinullification leaders in the South sought to confine the nullification dispute to the issue of South Carolina's challenge to the Constitution and the Union. Some sought to entice Webster into an involvement in Georgia's parallel *de facto* nullification of a Supreme Court edict barring the removal of the Cherokee Indians; others sought to widen the dispute to a debate over slavery. Webster responded that slavery was an issue for individual states to decide and assured the unionists of South Carolina that there would be no "further difficulty with Georgia."

This volume of *Correspondence* closes in 1834, with Webster trying vainly to find a way to resolve the dispute over the Bank of the United States and seeking to avoid an open condemnation of Jackson's use of executive authority. Webster's model of political conduct was the leadership of 1787, when the "giants" of the early Republic resolved disputes as creative statesmen should. He lamented to a friend that "This is a day of pigmies." Ultimately Webster had no choice other than to join the Whig party's unifying denunciation of Andrew Jackson's "arbitrary power." Yet he also

continued to believe that the country yearned for "distinguished men" at the helm of government and would place him there on that account. Whether Webster calculated accurately is the subject of the next volume of this distinguished series.

SYDNEY NATHANS
Duke University

IRVING H. BARTLETT. *Daniel Webster*. New York: W. W. Norton. 1978. Pp. xii, 333. \$12.95.

Representative William Lowndes Yancey of Alabama declared in a House speech in 1846 that Massachusetts Senator Daniel Webster had "two characters which, Proteus-like, he can assume as his interests or necessities demand—the 'God-like' and the 'Hell-like'—the 'God-like Daniel' and *Black Dan*!" Irving H. Bartlett's biography is an attempt to understand the Black Dan—godlike man paradox along with the inner man behind the dual images. He believes that much of the ambivalence attached to Webster's name can be explained by the fact that he operated on two separate levels simultaneously. "As a symbolic leader, guardian of the people during a period of tumultuous change, he was perceived as patriotic, disinterested, statesmanlike" (p. 194).

This perception inevitably clashed with Webster's role as an ambitious politician trying one expedient after another to broaden his political base and win the presidency. The Democrats never tired of reminding the voters that the champion of the Constitution and the Union had been a partisan Federalist congressman during the War of 1812. The weaknesses in his character—he drank and ate to excess, spent money recklessly, and was chronically dependent on powerful creditors like the National Bank—troubled even close friends. The Seventh of March speech in support of the Compromise of 1850, his final effort to eliminate the slavery issue from national politics, enraged New England antislavery men and lent further substance to the Black Dan image. Lamented John Greenleaf Whittier in "Ichabod": "All else is gone; from those great eyes/The soul has fled:/ When faith is lost, when honor dies/The man is dead!"

Bartlett traces many of the apparent contradictions in Webster's character to his close relationship with his father Ebenezer, a New Hampshire tavernkeeper, farmer, and local politician. Internalizing Ebenezer's unfulfilled ambitions, Webster mixed them with his own to surpass his father; yet at the same time he remained emotionally dependent on him. "Excellent, excellent parent! I cannot think of him, even now, without turning child again," Webster wrote in 1829, when he was at the height of his powers.

Since Claude M. Fuess's two-volume biography was published in 1930 and Richard M. Current's excellent brief life in 1955, four specialized monographs have appeared; but a modern biography, making use of the microfilm edition of the Webster papers, has been needed. Bartlett has provided a gracefully written, psychologically insightful study that will take its place as the standard life. I wish, however, that he had made more use of the unpublished papers of other political leaders and had treated Webster's public career in fuller detail. The larger-than-life, enigmatic man who played a dominant role in American politics, law, oratory, and diplomacy for forty years cannot be encompassed within 295 pages of text. "This colossal task of *Webster*," as Gamaliel Bradford wrote despairingly early in the century, remains unfinished.

NORMAN D. BROWN
University of Texas,
Austin

JERRELL H. SHOFNER. *Daniel Ladd: Merchant Prince of Frontier Florida*. Gainesville: University Presses of Florida. 1978. Pp. ix, 180. \$8.50.

Contrary to the import of the title, this work is not a biography of Daniel Ladd. Indeed, Ladd does not become a central figure until the fourth chapter. Due to the absence of a repository of Ladd's personal papers, Jerrell H. Shofner is unable to provide insights into his subject's personality, character, and motivations. The sparsity of information is most clearly evidenced by the author's inability even to decipher, except in the most cursory way, Ladd's position on the slavery question. The work, which also lacks continuity and critical analysis, is further marred by the inclusion of trivial economic statistics, tedious digressions, and repetition. No meaningful attempt is made to relate the activities of Ladd to those of his business contemporaries. The fact that the basic research is thorough leads one to suspect that the surviving material on Ladd is simply not sufficient to warrant a full-length study. The work emerges as neither a biography of Ladd nor an economic history of middle Florida, though it combines elements of both types of studies.

This volume, despite its major shortcomings, contains informative material for the serious student of the South. Ladd represents a segment of Southern society that has been largely ignored and greatly misunderstood. A native of Maine, Ladd became a wealthy entrepreneur in frontier Florida where he resided from 1833 until his death in 1872. Although his primary occupation was that of a cotton factor, Ladd engaged in a variety of enterprises including the operations of a turpentine distillery, cotton press, toll road, ice business, and

steamboat venture. Most of his activities were along the St. Marks River in the vicinity of Newport. The author seems to depict Ladd as being extremely cautious in his approach to social and political problems. Ladd apparently exhibited consistent materialism and, in the face of adversity, imagination and a shrewd business acumen. By recounting Ladd's financial transactions, the author is able to provide some important detailed information about the workings of the cotton factorage system. In several random passages Shofner chronicles aspects of the economic development of middle Florida and south Georgia.

While certain contributions are made in the work, one hopes that its organization and style will not serve as models for future publications on Southern businessmen. The development of a central theme or thesis, a more thorough analysis of economic data, the inclusion of comparative material, and the omission of extraneous information would have significantly increased its value.

ROBERT NEIL MATHIS

Stephen F. Austin State University

MARIE CASKEY. *Chariot of Fire: Religion and the Beecher Family*. (Yale Historical Publications, Miscellany, number 117.) New Haven: Yale University Press. 1978. Pp. xv, 442. \$25.00.

Paxton Hibben in his 1927 biography of Henry Ward Beecher managed to produce a striking portrait of Elmer Gantry. In explaining how his subject came to that bad end, Hibben also depicted a tyrannical father, Lyman, whose only sincere concern in a long and fraudulent life was to punish deviations "from the stern Calvinism of Jonathan Edwards" (Hibben, p. 8).

Marie Caskey, in an able and often fascinating study, now elaborates the confusing truths that lay behind such fictions. Lyman, the old heretic who considered Edwards's *Religious Affections* part of "a bad generation of books," was neither a model Calvinist nor especially stern; but his power over his children was very real. Significantly for the liberal evangelicalism to which he and most of the younger Beechers contributed, the instrument of his dominance was an unrelenting concern about their conversions and their godly vocations. The responses of the offspring consisted not in definitive rebellions but rather in the varied combinations of *Angst*, activism, and occasional madness that inform the Beechers' publications (over one hundred books by daughters, sons, and spouses) and their vast correspondence. These were not troops goaded to mutiny; the image is that of a high-spirited, ragtag platoon in which awkward recruits break step and march off at angles or to the rear—at no time ceasing their debates with

each other or losing contact with their anxious, cajoling lieutenant.

Caskey's efforts to impose order are, excusably, not much more effective than poor Lyman's were. After a fine chapter on the family setting and a less original one on the father's theology, she devotes a chapter to each of the seven most interesting and productive children, then concludes with essays describing the Beechers' involvement with Spiritualism and summarizing a varied family theology. The seven who produced further alleviations in Lyman's "alleviated Calvinism" are classified as moralists (Catharine and Isabella), prophets (Edward and Charles), and Christocentric liberals (Harriet, Henry, and Thomas). These characters, as the author notes wistfully, insisted on marching in and out of each other's categories, or occupying all three at once; and she reacts not only by making little of the classifications, but also by revealing little of the cultural and theological significance of each.

More general but less assailable is Caskey's overall conclusion that in an age perfectly willing to think anthropomorphically the Beecher theologies converged in a picture of God and divine government that reflected Lyman's own attributes and his mode of caring for his children. The Deity, "supremely paternal, could not be vindictive or hard-handed" (p. 381). When rebellious children ignored commands, the parent-God with infinite patience called them back. "There was no hint in Henry's sermons that the Father would be angered and abandon the child, for divine love was unconditional." Caskey's principal conjecture about long-range cultural influence is that the Beechers helped make this parental conception "a permanent element of evangelical religion in America" (p. 383).

The strengths of the book lie in its exhaustive and careful use of the family correspondence, its convincing blend of sympathy and acute criticism, its reticence in psychological speculation, and Caskey's generally splendid writing. The author conveys skillfully those amazing, fertile combinations of messianism, censoriousness, humor, introspection, human and social concern, and non-conformity that marked Beechers as individuals and as America's longest-running encounter group.

The major weakness stems from Caskey's allowable decision to stress the Beechers' struggles with their own and each other's agonies of faith. Her subtitle (despite opposite implications in the title) might have been "*Personal Religion and the Beecher Family*." We read relatively little about the entanglements of Edward or even of Harriet in antislavery, or about Catharine's educational reforms and almost nothing about the remarkable accomplishments and popularity of Thomas K.

Beecher, the maverick founder of the institutional-church movement. One will turn for those segments of the family history to Lyman Stowe's *Saints, Sinners, and Beechers* (1934) and to recent individual studies. But this reader would gladly have sacrificed some of the overlapping theological analyses for more of Caskey's expert guidance through the puzzling connections between personal religious struggles and public careers. When the format does prompt her to offer that, as in the case of the supremely messianic Isabella, the results are not merely more satisfying; they are genuinely brilliant.

WILLIAM R. HUTCHISON
Harvard University

SUSAN SUTTON SMITH and HARRISON HAYFORD, editors. *The Journals and Miscellaneous Notebooks of Ralph Waldo Emerson*. Volume 14, 1854-1861. Cambridge, Mass.: Belknap Press of Harvard University Press. 1978. Pp. xxvi, 523. \$37.50.

This volume continues the edition begun in 1960. About three hundred and seventy pages comprise the five regular *Journals* kept between 1855 and 1861. The next sixty pages are devoted to the notebook "WO Liberty," compiled, the editors believe, between 1854 and 1857 from material for antislavery addresses. The last fifty pages of text contain the five pocket diaries used on lecture tours from 1856 to 1860. Needless to say, all this material is elaborately and meticulously edited, making one question sometimes whether the apparatus is justified and whether it helps or hinders comprehension. Reading the volume is frequently like picking blueberries overgrown with catbrier.

Nevertheless, the picking is worth the pain. The consensus view that in these years Emerson could not sustain his belief in freedom and lost his romantic daring definitely needs qualification. In the antislavery notebook, found in the Library of Congress in 1964, he is particularly strong, sounding more like Thoreau than like an aging moralist and squire of Concord. The man who wrote in 1855 that higher laws are not revealed "in societies . . . but to private persons" was still a vigorous American radical (p. 397). He decided he was willing to "undermine institutions" if that was what opposing slavery led to (p. 383) and instructed himself, "Do not be so afraid of Anarchy" (p. 402). "Massachusetts, in its great day, had no government: was an anarchy . . ." (p. 407).

Yet he continued to be a man of contradictions; sometimes he was difficult, sometimes endearing. In 1859, preparing for a speech at a dinner honoring Oliver Wendell Holmes, he made the measured judgment that Holmes was "not a heavy

man nor a heavy companion nor a heavy writer" (p. 313). A few pages later, he drafted the speech, laboriously praising Holmes for his wit, "his delight in manners, in society, in elegance, in short . . . his delight in *Culture* . . ." (p. 316). Then, a few pages further, he quoted Dr. Johnson, "Bolder words & more timorous meaning, I think were never brought together" (p. 318). He directly applied the quotation to the sermons of James Walker, then president of Harvard, but I suspect that he was also thinking of his own sociable insincerities. His relations with Holmes and the other members of the Saturday Club gave him pleasure, but he seems to have been too shy and rigorous ever to relax fully. For how could he make polite chitchat when he was also telling himself that "things said for conversation are chalk eggs" (p. 254).

In short, what appeals in this volume is the freshness and nearness of Emerson the person. A man so reserved and scrupulous is only to be known in his private *Journals*. That his earlier editors Edward Waldo Emerson and Waldo Emerson Forbes made him less of a person is well known. This latest volume furthers the restoration of his wildness, his uncertainties, and his originality.

ROBERT F. SAYRE
University of Iowa

ROBERT DOHERTY. *Society and Power: Five New England Towns, 1800-1860*. Amherst: University of Massachusetts Press. 1977. Pp. viii, 114. \$10.00.

All historians of American communities wrestle with the same dilemma: should one write another case study, gaining precision from the rich sources of local history; or should one write a comparative history, gaining perspective by suppressing local detail? Given human weaknesses, the dilemma is insoluble. But worrying about it has produced a number of "outward-looking" local studies, explicitly aimed at comparison with each other. Now, in Robert Doherty's admirable and pioneering work, we have a "localized" comparison, in which the study of general trends is enhanced by attention to local circumstances.

Doherty opens his work with a summary of recent questions about antebellum society—questions concerning the extent of geographical mobility and its consequences, the distribution of wealth, the stability of elites, and the "openness" of society. Using a large file of census and tax data, he confirms, extends, and refines many recent answers. Like others, he suggests that transience increased, wealth became more concentrated, prospects for individual improvement were moderate at best and non-existent for many, and municipal

officeholding ceased to be a preserve of the wealthy.

Doherty's greatest contribution, however, is to demonstrate that these findings are indeed generalizations, masking a wide range of variation among communities. In considering every question about society, he finds that "the answer depends on where you look." His sample of five Massachusetts towns includes two remote hill communities (Pelham and Ware), two marketing and administrative centers (Worcester and Northampton), and one seaport (Salem). Between 1800 and 1860, these five experienced different growth rates, developed different social structures, and came to occupy different places in an economic hierarchy. Geographic and social mobility carried people away from lesser hamlets and toward the centers of growth. Local economies determined the extent of individual opportunity. Changes in officeholding were a function of community size and previous history.

This is a frankly exploratory study, a "progress report" in the author's words. Doherty's candor and modesty about his material make his book a pleasure to read and a useful teaching resource. Specialists, however, will find that it suffers from the weaknesses of many comparative studies: a reliance on statistical "common denominators," and a sketchy, typological use of local history. Doherty attempts to trace changes over six decades, but most of his data are taken from the first and the last. He provides portraits of his towns in the Jeffersonian era and in the chaotic 1850s. Only a schematic outline fills the intervening forty years—years which contained two wars, two depressions, an explosive growth of railroads, massive immigration, and a restructuring of politics. The patterns of the 1850s differ from those of the 1800s, but we do not know whether the differences reflect secular trends or peculiarities of those decades. This lack of continuous data, combined with thin local history, also weakens Doherty's claim that these towns held "two separate populations" (p. 44), a stable core and a floating mass. Other recent research (by Don Doyle, Michael Katz, Tamara Hareven, John Modell, and Lynn Lees) has suggested the capacity of voluntary organizations and "malleable" households to integrate migrants. The relationship between stable and transient people deserves closer attention. Finally, Doherty's equation of officeholding with "power" leads him to questionable conclusions. Did propertyless city councilors of the 1850s truly have any more power than propertyless men in the town meetings of an earlier day? Statistics on officeholding will not provide an answer, and Doherty's work lacks the detailed local study that would.

HENRY BINFORD
Northwestern University

LARRY D. BALL. *The United States Marshals of New Mexico and Arizona Territories, 1846-1912*. Albuquerque: University of New Mexico Press. 1978. Pp. x, 315. \$12.50.

The history of territorial government in the United States has traditionally received little attention. The traditional view is that frontier citizens deeply resented an arrangement imposed upon them by the federal government and felt that the system was undemocratic, incompetent, inefficient, and often corrupt. Fortunately, some less-biased, better-balanced views are being aired. The major need has been and remains area-by-area studies of government as applied to the middle and far West in the nineteenth century. This volume is an effort to explore one facet of the pre-statehood governments as found in two southwestern territories.

In most instances so little is known about territorial government that an author can assume that there is no general misunderstanding about the subject. Unfortunately, United States marshals regularly roam television and motion picture screens providing abundant misconceptions. To add to the confusion some towns in the West called the policeman a marshal. Thus it is helpful that the book explains the theoretical role of the United States marshal and his duties and place within the political arena. The author then devotes most of the rest of his work to a chronological narrative about New Mexico and Arizona marshals, their deputies, and their adventures. A brief conclusion attempts to synthesize and summarize a story not easily reduced to themes and threads of history.

This volume demonstrates the great differences between the histories of Arizona and New Mexico. More attention is paid to the latter, probably because of its longer history as a territory but also because more basic research has been done on the one and not the other. The colorful characters and personalities of the marshals are main themes although the institutional approach is not neglected. Some of the office holders tended to blur the distinctions between a lawman and an outlaw. Larry D. Ball mentions that Secretary of the Navy William E. Chandler found Marshal Tidball of Arizona "most capable and deserving." The author does not explain that Chandler's recommendation was influenced by a reminder from the would-be appointee that he deserved office because of his highly successful tampering with the ballot box in Florida during the disputed presidential election of 1876! Nor does Ball mention that during the uproar over the appointment of Benjamin F. Daniels by Theodore Roosevelt one of the complaints was that Daniels's principal occupation was that of faro dealer in a Nogales casino.

Research for this book was rather extensive, although perhaps limited to the more readily avail-

able materials. A few relevant sources were omitted and more material could have been found by additional work. Errors are at a minimum and consist largely of wrong middle initials and the like. Some operations and relationships within territorial government are not as fully explained as they might have been, but on balance the book is valuable and likely to remain so. Heretofore, there has been no comprehensive publication on the subject.

JOHN S. GOFF
Phoenix College

DONALD C. BIGGS. *Conquer and Colonize: Stevenson's Regiment and California*. San Rafael, Calif.: Presidio Press. 1977. Pp. xx, 263. \$12.95.

To most of us who have perused the story of the California Gold Rush, the term "New York volunteers" will conjure up the image of organized hooligans terrorizing the city of San Francisco. The discharged soldiers were assumed to have retained enough of their military team-spirit to have continued acting together as a violent gang called "the Hounds." Their chief historical importance has been seen as the precipitating force behind the Vigilante committees.

Donald Biggs has looked into this matter in a more thorough fashion, and has emerged with a very different image of Stevenson's Regiment. Organized under the directive of President Polk, the regiment of volunteers was intended not only to contribute to the conquest of California but also to stay in the new territory and help settle and develop it. Biggs demonstrates that the men of the regiment performed well the task assigned them. His research is impressive. Working with army records, contemporary accounts, and a good deal of newspaper coverage, he has given an almost man-by-man survey of the regiment's personnel from the time of recruitment through the voyage round the Horn to the deployment in California. Though the conquest of California was achieved even before they arrived, there was some mopping up to do. The major job of colonization remained, and in this the discharged men made a major contribution.

If the book has a fault, it is that the author's objective of rehabilitating the character of the regiment and of stressing the good conduct and useful services of most of its members absorbs almost all of his attention. The less savory part of the story is passed over with a few generalities. The source material for the violence and misconduct of the few—though voluminous—is not even included in the otherwise admirable bibliographical essay.

For all that, this is a useful and instructive work,

thoroughly researched within the limits the author set for himself. The text is highly readable and it is set off with well-chosen contemporary illustrations.

EDWIN A. BEILHARZ
University of Santa Clara

P. R. D. STOKVIS. *De Nederlandse trek naar Amerika, 1846-1847* [The Emigration of the Dutch to America, 1846-1847]. (Leidse Historische Reeks, number 21.) The Hague: Martinus Nijhoff, for Universitaire pers Leiden. 1977. Pp. viii, 251. f 46.80.

The story of the "Great Migration" of 1846-1850 that brought some 12,000 Dutchmen from the Netherlands to our shores and led to the founding of Holland, Michigan, Pella, Iowa, and other settlements in the Middle West has been told many times by authors on both sides of the Atlantic. A young Dutch scholar has now re-examined the subject in a monograph designed to ascertain the relative impact that each of several factors—economic, social, and psychological—had on the emigrants' decision to pull up stakes and move to the U. S. The study focuses on the two critical years of 1846 and 1847, when a distinct pattern of migration was established and the influx from the Netherlands reached its first peak.

Having thus limited the scope of his investigation, the author has explored the relevant sources in depth and with considerable success. He has made particularly effective use of contemporary statistics and has thus been able to define more accurately the part played by the so-called "Seceders" in this migration. These religious fundamentalists made up 35 percent of all Dutch emigrants in 1847; they also gave leadership and form to the movement. But they remained a minority, and the author proves conclusively that the emigration of 1846-47 sprang primarily from social and economic considerations. Religious motives played their part but they did not dominate, as some writers have maintained.

Drawing upon archival sources in both the U. S. and the Netherlands, the author supplies a wealth of interesting and frequently new information on the organization of the emigration, the attitudes of the U.S. and Dutch governments, and the system of trans-Atlantic shipping that made such mass migration feasible. American diplomatic and consular personnel in the Netherlands are severely criticized for their failure to provide accurate and timely information concerning regulations passed by Congress in February 1847 to prevent overcrowding of emigrants on merchant vessels. Though our representatives did not exactly cover themselves with glory on this occasion, certainly

the blame for this diplomatic foul-up, and the temporary inconvenience it caused to shippers and emigrants alike, should be shared by the Dutch diplomatic officials in Washington, who waited over a month before even reporting the new legislation.

Other chapters deal with the image of the U.S. in the Netherlands and with the public commentary on the emigration in contemporary Dutch pamphlets and newspapers. Though based on original research, these sections reveal little that was not known before; but they often add new and interesting illustrative material. Despite a rather heavy-handed methodological approach and an occasional simplistic generalization—trans-Atlantic migration is said to have been the historical reason for the existence of the United States—the book has great merit. As a topically arranged compendium of carefully researched data on the Dutch background of immigration from the Netherlands in 1846–47, it is likely to be useful for a long time.

BERTUS H. WABEKE
Alexandria, Virginia

GERALD W. WOLFF. *The Kansas-Nebraska Bill: Party, Section, and the Coming of the Civil War*. (Studies in Nineteenth Century American History.) New York: Revisionist Press. 1977. Pp. vii, 380. \$49.95.

In all of the extensive writing about the Kansas-Nebraska Act, Gerald W. Wolff points out, not enough has been done to delineate the actual voting patterns on the bill and other legislation in both houses of Congress or to measure systematically the actual extent of sectional as against partisan pressures on legislative behavior in 1853–54. Wolff, therefore, has applied quantitative analysis, specifically Guttman scaling, to encompass the behavior of all of the congressmen, the inarticulate as well as the prominent, not only on each bill's final passage but also on all the amendments and procedural moves that preceded final action. He offers an enormous storehouse of data for analysis, far surpassing anything we have had before on this matter. One-third of the book consists of charts, appendixes, and methodological explanations. In addition, he has gone through the debates, newspapers, and manuscripts searching for background, motivation, and explanation. With this material as his base, he then assesses the relative importance of different influences on congressional voting behavior.

Wolff presents his results in a straightforward, clear, and controlled manner. He does not claim more for his data or methods than they can bear. He addresses the specific findings of others from Nichols and Russel to Alexander, revising, correct-

ing, and offering alternative explanations as appropriate. He draws two major conclusions from the patterns in his 396 roll-calls. The first is that "hostilities between North and South and disunity within political parties were not as all encompassing or dominant as one might be led to expect" (p. i). Sectional influences were present but party discipline held more firmly than anticipated, especially since President Pierce effectively used his patronage powers among northern Democrats. Second, whatever the impact of Kansas-Nebraska in exacerbating sectional tensions (especially within the Whig party), its impact on other legislation was nil. On tariffs, railroad grants, internal improvements, and foreign affairs, older influences, local, regional, and especially partisan, shaped congressional voting. This was a transitional political period but there was "a diversity of determinants" operating in Congress despite the highly charged sectional atmosphere usually emphasized.

Wolff offers few startling revelations. But he has effectively nailed down a number of disputed points, corrected emphases, and offered solid evidence for settling other matters. More might have been done to strengthen the analysis. The argument would be greatly enhanced by some comparative assessment of the different voting determinants with earlier periods to evaluate how much things may have moved, toward sectionalism for example. There was such a change, as Wolff recognizes, and that needs to be evaluated by moving outside the microscopic chronological and institutional confines of the Thirty-third Congress. Further, his analytic categories, drawn from the scales, might have been cast differently to reveal more subtle behavioral shadings. But these are suggestions for further research. Wolff's virtue is that he has provided a useful starting point and a body of data for wide-ranging examinations of a crucial moment.

JOEL H. SILBEY
Cornell University

MICHAEL F. HOLT. *The Political Crisis of the 1850s*. (Critical Episodes in American Politics Series.) New York: John Wiley. 1978. Pp. xix, 330. Cloth \$10.95, paper \$6.95.

In recent years, studies of the politics of the 1850s have followed one of two interpretations. Some historians explain the disruption of the second party system by the intrusion into politics of rival sectional ideologies, differing above all in their attitude toward black slavery. A second group, the "new political historians," argue that at the local level such issues as temperance, nativism, and con-

flict between ethnic and religious groups had more salience than slavery in affecting voter loyalty and in contributing to political disruption.

In this study, Michael F. Holt advances a new way of looking at the politics of the 1850s. The author of a widely praised book on the emergence of the Republican party in Pittsburgh, Holt accepts the centrality of the slavery issue to the sectional conflict. He insists, however, that longstanding sectional hostility cannot explain why the disruption of the Union occurred in 1860–61, rather than 1820, 1850, or some other year. At the same time, he believes that the ethnoculturalists have not explained why the Republicans, and not the Know-Nothings, emerged as the leading alternative to the Democracy.

In Holt's analysis, the party system itself moves to center stage. The political crisis of the 1850s, he believes, was an independent development, with causes lying outside the sectional controversy. Holt contends that until the early 1850s the system was able to survive the shocks of sectional discord. Far from excluding ideological issues, indeed, politics thrived on sharp debate between the parties. What undermined the system was not conflict, but consensus. The Compromise of 1850 eliminated debate on slavery, while economic development made many of the old financial issues obsolete. As a result, voters no longer perceived any differences between the major parties, and the result was a "loss of popular faith" in the parties' ability to "meet the needs of voters."

This widespread distrust of parties and politicians was the real "political crisis" of the early 1850s. It was not the intrusion of sectionalism that destroyed the party system but rather collapse of the parties that unleashed sectional antagonism.

Probably the most original part of the book is Holt's discussion of the collapse of the Whig party, a discussion central to his entire thesis. His analysis of the election of 1852 is highly original, emphasizing not the magnitude of the Whig defeat, but the decline in voter participation, an indication of broad dissatisfaction with both parties. Moreover, Holt stands the ethnocultural interpretation on its head, arguing that what doomed the Whigs was not nativism, but their attempt in 1852 to woo the Catholic immigrant vote, thus destroying the last party position distinguishing them from the Democrats.

In contrast to historians who emphasize conflict between deeply antagonistic sectional ideologies, Holt insists that a common political outlook was shared in North and South—republicanism, the heritage of the American Revolution. It was hostility to slaveholders as an "unrepublican" oligarchy, rather than defense of the free labor system of the North, which lay at the heart of the

emergent Republican party. Indeed, Holt argues, when Northerners spoke of slavery, they often had in mind not the bondage of the black, but a classical republican paradigm in which "slavery" denoted a more general threat to individual liberty.

The idea that republicanism persisted into the 1850s as the organizing theme of political life is an intriguing one, and Holt demonstrates that it affords new insights into the political debate of these years. Unfortunately, he does not seem to have familiarized himself with the extensive literature on the social and cultural dimensions of republican thought. As a result, he never defines precisely what he means by "republicanism," aside from a commitment to popular control of the government.

Based on an extensive reading of secondary sources and his own research in newspapers, manuscripts, and voting statistics, the book advances a host of original interpretations of this oft-studied period. In a field in which historians' views often reflect the concerns of their own time, Holt's description of the collapse of a political system in which parties and politicians were viewed as unworthy of popular confidence is perhaps singularly appropriate for the post-Watergate era.

ERIC FONER
City College,
City University of New York

RAIMONDO LURAGHI. *The Rise and Fall of the Plantation South*. New York: New Viewpoints. 1978. Pp. 191. Cloth \$12.50, paper \$5.95.

In this slender volume Raimondo Luraghi of the University of Genoa presents conclusions drawn from twenty years of study of the American Civil War. This recognized authority's thesis is that the fratricidal clash was a result of an inevitable conflict (although he does not use that term) between two competing cultures. In his words, "the most dramatic contrast between the South and the North was neither economic nor moralistic: in fact, it was ideological. At its heart, it was still the sharp contrast between an Elizabethan, classically minded, aristocratically individualistic South and a puritanical, trade-minded, Calvinistic North" (p. 62).

His South was a "seigneurial" society and a genuine product of the Americas. Its roots were bedded in Greece and Rome and were nourished by the classicism of the Italian Renaissance. Sir Walter Raleigh transplanted its seed, and Virginia became the legitimate flower of the English Renaissance. The seigneurial concept, with its goal of "status, power, and the good life, not accumulation" (p. 47), established itself in both Americas and "succeeded to a remarkable degree in creating

a 'culture' which . . . is still among the richest and most fascinating of the world" (p. 6). It was not capitalistic nor was it dependent upon slavery or "single-crop" agriculture, although it was shaped by all three.

Its rival in North America was created by the settlement of New England and the Middle Colonies by groups that were anti-classic. Their ideals were bourgeois and capitalistic, creating a confrontation that can be viewed as Raleigh versus Calvin or a migration of the old struggle of "saints" and "demons" across the Atlantic. After the Industrial Revolution swept Europe and the North, the latter could no longer allow the southern seigneurial class to go its separate way. Confronted by growing northern avidity for markets and labor, the South finally was faced with yielding or developing a nationalism of its own. Choosing to resist, it created the Confederacy, a "kingdom of utopia" that was destined for defeat from its very birth. Acceptance of a type of war that it was not prepared to fight—industrial warfare—convinces Luraghi that southern leadership had a premonition that all it stood for was doomed and that it really had only the choice of the best way to die.

Professor Luraghi's arguments are buttressed by citations from a wide variety of American archives and contemporary international scholars. Aside from his conclusions, the most interesting part of his work is his comparison of the southern seigneurial class with similar groups in other nations. He writes well, and his cogent presentation carries conviction, although his stated objective is to drive "people to challenge my views—and by this, to increase and deepen the research in this field" (p. 8). Whether one ends by challenging or accepting his premise, the reading of this work will be worthwhile.

JOHN S. EZELL
University of Oklahoma

DENA J. EPSTEIN. *Sinful Tunes and Spirituals: Black Folk Music to the Civil War*. (Music in American Life.) Urbana: University of Illinois Press. 1977. Pp. xix, 433. \$17.95.

Through an examination and evaluation of travel accounts, memoirs, reports of ship captains, letters, abolitionist literature, church histories, novels, and a few selected slave narratives, Dena J. Epstein, a University of Chicago librarian, provides extensive documentation and analysis of contemporary descriptions of Afro-American music from the early colonial period to the end of the Civil War. Where North American sources are

sparse, particularly for the seventeenth and eighteenth centuries, she turns to accounts from the British and French West Indies on the sensible assumption that during the early period, when the acculturation of Africans into New World societies had not progressed very far, the music of the island slaves and those of the North American mainland was probably similar.

While Epstein's work complements recent interpretations of slave music by John Lovell, Sterling Stuckey, and Lawrence Levine, as well as brief but perceptive accounts by John Blassingame, Leslie Owens, and Eugene Genovese, she largely eschews interpretation of the meaning of slave music. She concentrates instead upon describing the form, content, and use of black music as depicted by contemporary observers. Since the overwhelming majority of the observers upon whom she relies were Europeans and white Americans, Epstein is acutely aware of the ethnocentric, even racist, perspectives which pervade contemporary accounts. Inclined to view non-European music as "exotic manifestations of the primitive mind, barbaric and strange," few white commentators understood African and Afro-American music and fewer still appreciated it. Not surprisingly, those who were most sympathetic to Africans and slaves as human beings were likely to exhibit the greatest appreciation for slave music. Fortunately, those who edited *Slave Songs of the United States* (1867), the first published collection of Afro-American music, were not only in this category, but also were people whose education and broad cultural interests enabled them partially to transcend their ethnocentrism.

Despite their deficiencies, the contemporary accounts lead Epstein to several well-supported conclusions: (1) African percussive, string, and wind instruments, such as the drum, the banjo, the balafon, the flute, the musical bow, and the panpipe, were brought to the Americas by or with the slaves and were often reproduced in America by them; (2) early slave music was distinctively African, and Afro-American music retained strong, even dominant, African features even after it became fused with European models and themes; (3) secular music was as common as the spiritual, with which it shared stylistic and utilitarian motifs; and (4) slaves used music for a variety of purposes: to celebrate and entertain, to worship, to ease the burden of work, and to make aspersive comments about members of the master class.

While these observations are by no means original, Epstein provides fuller documentary support for them than heretofore existed elsewhere. Her text, her "Additional Sources" at the end of each chapter, and her appendixes contain extended extracts from her sources. Nevertheless, she acknowledges that much more needs to be done, especially

in searching out descriptions and interpretations of slave music by the slaves themselves.

ARNOLD H. TAYLOR
Howard University

JAMES H. MOORHEAD. *American Apocalypse: Yankee Protestants and the Civil War, 1860-1869*. New Haven: Yale University Press. 1978. Pp. xiv, 278. \$17.50.

The Civil War erupted as a defense by Southerners of their right to self-determination; it became a crusade to purify the Republic, indeed, perhaps even to redeem human history itself. Through the necessity for self-justification, the habit of dramatizing history—and especially American history—as an unfolding of God's purpose, and the inexorable cadence of the "Battle Hymn," a vision of ultimate judgment was drummed into the minds of thousands of Yankees. The meaning of that process—what it tells us about American nationalism, Yankee Protestantism, and middle-class ideology in the nineteenth century—is what this impressive, intelligent, lucid, and well-argued book is all about. Sensibly avoiding discussion of a "civil religion" that transcended the churches, James H. Moorhead develops his thesis from the ideology of Yankee Protestant spokesmen, that fusion of religion and nationalism that ultimately set "the moral tone of the victorious union." The result was not merely "God is with us" sloganeering, but the perception, explanation, and celebration of the North as the Republic, and the Republic as God's redeemer nation. Before the war, Yankee Protestants could not decide "whether the American promise was a sacred polity to be preserved or a higher law to be obeyed . . ." (p. 17). During the war, however, the resolution of the dilemma became clear as sacred polity and higher law became the same thing. After the war, the two became even more closely identified with the Protestant bourgeoisie.

Sensitive to intellectual nuance and the subtlety of ideological formulation, Moorhead shows how Protestant theorists began to explain the war through the millennialist framework of judgment and renovation. It was punishment by a just God for sins of both North and South in renunciation of rigorous self-discipline and toleration of Afro-American slavery. As God's chosen people, Protestant Americans were being cleansed through the moral discipline of battle in which "national life was being regenerated in one decisive test" (p. 80). Although Moorhead's argument is weakened by failure to distinguish radical abolitionist ideology from that of non-abolitionist antislavery Protestants, it nevertheless lays bare an "ambiguous, even muddled" social analysis that never came to

grips with the structural and racist realities of American society. Before it was proclaimed, emancipation was seen as the apocalyptic act through which slaves could be freed from degradation, their former masters from licentiousness and sloth, and their Yankee friends from apathy and insensitivity to their role as God's instrument of justice. When the Thirteenth Amendment became law, however, the millennium had still not been ushered in. The dramatization of Northern morality had obscured the social problems inherent in abolition and "inflated" Protestant dreams "beyond the possibilities of realization" (p. 211). Failure of Yankee Protestant ideology to deal with the problems of slavery, abolition, and reconstruction resulted from its primary focus—the establishment of millennial order, the creation of "a disciplined and loyal citizenry who would not succumb again to the fractious ways of the antebellum era" (p. 161). Thus the destruction of slavery was punishment of white Southerners for attacking the millennial Republic more than it was the liberation of victimized blacks. And when the latter refused—it seemed—to act like white bourgeois Protestants within a few years of emancipation, Yankees "placed the stigma of his oppression upon the Afro-American himself" (p. 215). This scapegoating—resulting as it did from the view that freedom meant rigorous self-discipline and super-human effort—complemented very nicely the wartime fusion of civil and religious spheres when it became difficult to distinguish between political dissent and religious heresy. The authoritarian implications of an enforced conformity are clear; but the unity and order so desperately desired by the Northern Protestant bourgeoisie was beyond its power to achieve. The vast and accelerating changes of the late nineteenth century shattered the already subverted unity of American society and the millennial vision became trivialized in sentimentality, sobriety, and personal piety.

Moorhead's intelligent book avoids the murky abstractions afflicting much discussion of American nationalism; it reveals the limits of social radicalism when linked to demands for middle-class hegemony; and by implication it raises serious questions about prevalent theories of evangelicalism and social reform. If the author sometimes fails to clarify the social bases of the ideas he has studied, he nevertheless has produced an excellent book characterized by sophisticated insight and impressive analysis.

DONALD G. MATHEWS
University of North Carolina,
Chapel Hill

GEORGE P. RAWICK, editor. *The American Slave: A Composite Autobiography. Supplement, Series 1* in twelve

volumes. Volume 1, *Alabama Narratives*. (Contributions in Afro-American and African Studies, number 35.) Westport, Conn.: Greenwood Press. 1977. Pp. lviii, 499. \$325.00 the set.

In 1972 George P. Rawick edited and Greenwood Press published eighteen volumes of slave narratives, sixteen of which consisted of interviews with ex-slaves conducted under the auspices of the Federal Writers' Project in the 1930s. Although at first Rawick believed that these represented all the existing FWP narratives, he and his assistants have since discovered hundreds more that for one reason or another never reached the Library of Congress, where the original collection was deposited. (As Professor Rawick explains, there was evidently a conscious effort to suppress the narratives in Mississippi and Texas, but in most states the villain "was simply some administrative looseness and confusion . . ." [p. xvii].) The result of these new discoveries is a supplementary series of narratives, consisting of twelve volumes, with more planned for the near future.

The book under review, the first volume of the supplementary series, consists of one hundred and fifteen Alabama narratives, preceded by a useful fifty-one page general introduction to the series. Evidently Rawick is unaware that many of these accounts are somewhat different versions of interviews he published in the first Alabama volume (volume six of the original series). Despite his assertion that the newly published narratives "are all interviews with people not interviewed in the collection in the Rare Book Room" of the Library of Congress (p. lvii), comparison of the two volumes reveals that forty-five of the hundred and fifteen narratives represent interviews of the same ex-slaves, conducted by the same interviewers. In many cases someone has changed the wording of the narratives—quotations are paraphrased and events appear in different order—but the interviews are clearly recognizable as reworked versions of those already published.

On the whole, the newly published narratives have the same strengths and limitations as those historians have recognized in the earlier volumes and, when used with proper caution, will be of considerable benefit to historians of slavery. The supplementary Alabama volume, however, does have some distinctive characteristics that deserve mention. Most of the interviews, unlike those of some other states, are descriptive accounts, narrated by the interviewers, almost all of whom were white and most of whom seem to have been remarkably insensitive—at least by today's standards—to black feelings and attitudes. (In one case an interviewer appended a note urging the omission of a slave's "pre-posterous" reference to

being whipped for praying and singing [p. 434]). While previously published slave narratives contain sharply varying appraisals of life under the peculiar institution, the great majority of blacks in this volume describe their treatment as good and speak fondly of their owners. There are also fewer descriptions of slave resistance and less information on internal slave culture than those familiar with other narratives might expect. It should be noted, however, that these comments refer only to the Alabama volume which, by Rawick's own judgment, is far from the best. "Although it is a large collection and many items in it seem to be of considerable interest," he notes, "it is not a distinguished one. There are many items that are simply poorly done" (p. lvii).

The publication of these volumes, nevertheless, is an event of importance to students of slavery. Despite inherent problems with the narratives as sources, they constitute the largest body of information on slavery reflecting—at least in part—the perspective of the slaves themselves. As such, they are documents that no historian of slavery can ignore and will serve as fertile fields of research for generations of scholars. Rawick and his associates are engaged in a task of great significance, for which they have earned our gratitude. Perhaps in future volumes, however, they will eliminate duplication of already published narratives.

PETER KOLCHIN

University of New Mexico

WILLIAM H. ARMSTRONG. *Warrior in Two Camps: Ely S. Parker, Union General and Seneca Chief*. Syracuse: Syracuse University Press. 1978. Pp. xi, 244. \$11.95.

Except for Arthur C. Parker's rambling biography of his uncle, written in 1919, Ely S. Parker has remained a shadowy figure generally relegated to footnotes. Parker personified the educated Indian in the nineteenth century, and in this felicitously written study by William H. Armstrong, he at last receives some of the recognition he is due.

Born on the Tonawanda Reservation in western New York in 1828, Parker studied at Cayuga Academy and while there was chosen by the Seneca to defend them in Washington against the Ogden Land Company's claim to their reservation. The subsequent legal battles occupied Parker sporadically for two decades. For his efforts on behalf of his people, the Seneca made him a sachem. Impressed by the power of the law, Parker studied law but was denied admission to the bar because New York state law prohibited non-citizens from becoming lawyers. Parker turned to engineering, working first in New York and then in Galena, Illinois, where he met Ulysses S. Grant. The Civil

War strengthened their friendship, for Grant named Parker his personal secretary. When elected president, Grant chose Parker as his Commissioner of Indian Affairs. However, Parker ran afoul of the Board of Indian Commissioners, a board comprised of philanthropists appointed to review expenditures of the Indian Office and advise the government on Indian policy. The Board charged Parker with fraud and corruption, and although a Congressional investigation absolved Parker of the charges, he chose to resign. Upon leaving Washington, Parker engaged in several business ventures, but economic reverses plagued him, and he lived his last years in relative poverty as a clerk in the New York City Police Department.

Although Armstrong has combed the archives and researched all aspects of Parker's life, the result is somewhat disappointing. Armstrong's historical perspective is limited; he leaves too many basic questions unanswered and tends to focus more on anecdotes than on interpretation. It is, for example, too simplistic to see Parker's removal from office as a personal vendetta orchestrated by William Welsh, former chairman of the Board of Indian Commissioners. A series of misunderstandings over who had what authority precipitated the conflict, but its roots stretched back through the history of government graft and fraudulent dealings with the Indians that the Board had pledged itself to oppose. It is true that Parker was caught in the middle, but Armstrong does not sufficiently explore the historical context of the Board's well-intentioned concern for the Indians' welfare. It is also regrettable that Armstrong does not make a greater effort to delve beneath the surface and examine the inner stresses inherent in Parker's position between two cultures.

Although Armstrong has written an uncritical biography of Parker, he makes a valuable contribution in recording the changing attitudes of an educated Indian who tried to become a white man and who lived long enough to wonder if he had taken the correct course.

ROBERT E. BIEDER
University of Illinois,
Chicago Circle

GARY E. MOULTON. *John Ross, Cherokee Chief*. Athens: University of Georgia Press. 1978. Pp. ix, 282. \$12.00.

For nearly fifty years, from 1827 until his death in 1866, John Ross was the principal leader of the Cherokee Indians, a numerous and important tribe that occupied large areas of land coveted by southern planters. Ross was an enigma. He

boasted about one-eighth Indian blood, his command of the Cherokee language was less than fluent, and he was more comfortable rubbing elbows with the eastern elite than sharing the life of the Cherokee fullbloods who were his devoted followers. Nevertheless, he was an ardent nationalist who struggled to maintain the delicate balance between the needs of his people and white American demands. Indeed, his career spanned some of the most critical years in Cherokee history, including the tragic removal era. He tried to spare his people the agony of what became the "Trail of Tears," but even his statesmanship was no match for the iron-willed Andrew Jackson and his cohorts. The intra-tribal rivalries spawned by the removal controversy continued through the Civil War, which, for the Cherokees, literally was a war of brother against brother. Whatever the adversity, however, Ross shared the plight of his followers: his first wife, Quati, died of pneumonia en route to the West, and he lost his home and a son because of the war. Ross himself died in Washington during the protracted and bitter postwar negotiations between the federal government and the Cherokee nation.

Although there are minor typographical errors and factual errors (Brown's Hotel and the Indian Queen Hotel were one and the same), this biography is generally accurate and satisfactory. It is also disappointing. Because of the wealth of primary and secondary materials to draw from, the opportunity was here to produce a unique and important study instead of this barebones biography. Ross merits a book as rich and detailed as Thurmond Wilkins's *Cherokee Tragedy* on the Ridge family. The material for such a work was available to the author, who is also editor of the letterpress edition of the Ross papers. The author should also have examined the voluminous fiscal records of the Bureau of Indian Affairs, which are among the Records of the Second Auditor's Office in the National Archives. These might have been useful in explaining some of the complex financial manipulations that seem to have been so much a part of the Cherokee story.

HERMAN J. VIOLA
Smithsonian Institution

THOMAS DAVID SCHOONOVER. *Dollars over Dominion: The Triumph of Liberalism in Mexican-United States Relations, 1861-1867*. Baton Rouge: Louisiana State University Press. 1978. Pp. xx, 316. \$17.50.

Carefully researched and provocatively analytical books are always welcome, particularly when they overlay two fields. In this instance Thomas David Schoonover has provided such a volume linking

evolving Mexican-United States relations, during and immediately following the Civil War, with the domestic policies of the period. This is a carefully documented study of a six-year period within an interpretative frame and is certain to provoke discussion.

While employing the standard sources characteristic of diplomatic history, Schoonover also effectively utilizes documentary materials in public and private collections here and abroad and archival materials from Mexico and Europe as well as the United States. He traces relations between the United States and Mexico, covering the rivalry for position between Confederate and Union governments and their representatives, the attempts by Mexican Liberals and Union Republicans to build a new and friendlier relationship through treaties, Confederate relations with the Mexican Liberals and with the Imperial regime, the tangling of relations with the Civil War and the French invasion, and the untangling of those relations from those conflicts. Finally, the author examines the role of Mexican relations in postwar United States politics.

The author's thesis is that "the Liberal party in Mexico and the Republican Party in the United States shared an ideology—laissez-faire liberalism—which led them to define their respective problems and the solutions to their problems as interrelated and which alleviated some of the stress on United States-Mexican relations during the crisis years of the 1860s while permitting the mutually desired American economic penetration of Mexico during the 1860s and 1870s" (p. xix). He marshals evidence of the motives of political and business figures but has to admit that considerable weight must be given to national concerns regarding security and divergent political systems.

The weakest link is reading the narrowed liberalism of the Porfiriato back into this period. Certainly Mexican liberals wanted to develop and modernize their society, but it is questionable whether they were willing to do so at the price of economic dominance by the United States. There is much evidence of Mexican wariness and concern on this score. Furthermore, if Juárez and his Liberal associates had no concern for the popular mass, how does one explain the suspension of the Ley Lerdo when its unfavorable impact on Indian landholding villages became apparent? This reviewer suspects that the root of the problem is the author's failure to recognize that the Liberal leadership represented a political rather than an economic middle class.

In the final section of his book, Schoonover corrects two areas that he feels have been "ignored, slighted or misinterpreted" (p. 219) by previous researchers and writers. He argues that the French

intervention and Mexican affairs generally did affect United States domestic politics between 1865 and 1867. He concedes that by early 1867 with the Mexican issue apparently headed for a solution and domestic issues intensifying, the clash between Johnson and his opponents would center on their domestic difficulties.

Concern about preserving unrestricted United States access south of the Rio Grande reflected developing economic interests in Mexico during these years. The author details the efforts by "speculators, opportunists, capitalists and promoters" (p. 252) to gain concessions and investment opportunities. While their accomplishments admittedly were few and mostly minor, the author argues that the record is more active than usually credited and that the period marks a transition to the activity of the 1870s and beyond when, indeed, "dollars not dominion" would define United States relations with its southern neighbors.

STANLEY R. ROSS
*University of Texas,
Austin*

ALLAN PESKIN. *Garfield*. Kent, Ohio: Kent State University Press. 1978. Pp. x, 716. \$20.00.

James A. Garfield, born in backwoods Ohio in 1831, became a schoolteacher, president of Hiram College, a brigadier general in the Civil War, a Republican congressman for seventeen years, and president in 1881. Shot on July 2, 1881 and dying in September, he served actively as president only four months. Allan Peskin's account of Garfield is well documented, judicious, and absorbing.

Most people have looked upon Garfield as personable, well intentioned, moderate, intelligent, and eloquent, with an undistinguished record. Peskin confirms that impression and recognizes that on a few occasions Garfield misused his public office for private financial gain.

The success of this lengthy book is due in part to the author's skillful use of suspense. Garfield was caught up in one misadventure after another. Peskin neither moralizes about nor apologizes for Garfield's indiscretions, nor does he overpraise him. He makes no attempt to generalize in introductory paragraphs to chapters, doubtless because most events proceeded without focus or significance. Garfield drifted! This large-framed, handsome orator was a remarkable performer, but he had limitations that make clear why he could become president only by accident. He did not earn the honor through service to his party or nation, nor did he make a direct effort to obtain the post. In fact, he attended the 1880 convention as Senator John Sherman's manager and received

the nomination only because the delegates failed to agree on one of the leading contenders.

Garfield lacked ability for leadership. In Congress, Peskin points out, "Though he shone in debate, he was considered 'as helpless as a child' in plotting parliamentary tactics" (p. 434). Also, his ability to discriminate between right and wrong blurred, on occasion, when he needed funds to support his growing family. His involvement in the *Crédit Mobilier* affair raised serious doubts about him. Peskin states, "Was Garfield corruptible? Probably" (p. 362). In the DeGolyer scandal Garfield obviously "allowed himself to become trapped in a clear case of influence peddling" (p. 381).

Garfield only incidentally linked his political career to the desires of business interests. Peskin shows him to be a mild advocate of free trade, modified to please the iron producers of his congressional district. Like his 1880 opponent, Winfield Scott Hancock, he looked upon the tariff as a "local issue." Garfield for a time demonstrated an intense personal interest in the gold standard and crusaded for it without any apparent political motivation. But Garfield's campaign managers in 1880 made him realize that winning required a more pragmatic approach to politics, and he promised, albeit indirectly, to heed powerful railroad interests in making Supreme Court appointments. Campaign money then began to flow.

Peskin concludes that Garfield was a "misplaced intellectual thrown onto the stage of public life, moving restlessly between the worlds of action and introspection, . . . at home in neither" (p. 612).

HORACE SAMUEL MERRILL
University of Maryland

JOHN N. INGHAM. *The Iron Barons: A Social Analysis of an American Urban Elite*. (Contributions in Economics and Economic History, number 18.) Westport, Conn.: Greenwood Press. 1978. Pp. xix, 242. \$19.95.

The writing of group biographies, given strong impetus by the work of sociologist C. Wright Mills in the 1940s, has become an accepted technique of both historians and sociologists. John N. Ingham offers an analysis of 696 iron or steel manufacturers in five areas, Pittsburgh, Philadelphia-Bethlehem, Cleveland, Youngstown, and Wheeling, in the period 1874 to 1901. Their descendants are also traced for certain purposes down to 1965, making a total sample of 12,600 persons.

Pittsburgh's steel men are examined in more detail than those of the other five cities, which are used as comparative controls. The study examines ethnic and family backgrounds, education, mar-

riage, neighborhoods lived in, and participation in social organizations. Ingham reaches four conclusions: first, businessmen were "social animals" whose nonbusiness concerns affected their economic activities; second, the entrepreneurs studied were members of a group or class; third, the upper-class businessmen and their families tended to remain community- and place-oriented; and fourth, "there has been more continuity than change among the business elites and upper classes in America."

Granting that Ingham's research is properly designed and accurate, the reader should still have some reserve as to how much such studies can demonstrate. How typical were these businessmen? Iron and steel rapidly became one of the most concentrated, or big business-dominated, of American industries, and all manufacturing, in turn, has represented only about a quarter of business activity. Thus this study is slanted by its material to an analysis of how men succeeded in a bureaucracy, whereas 90 percent or more of American entrepreneurs did not face this problem. Ethnicity, for example, had a different relation to success in the majority fields of small- to medium-sized business than in a WASP-controlled hierarchy.

Another problem underlying all group biographical studies is the circularity of cause and effect. Did a man seek admission to a somewhat exclusive club to advance his business prospects or because membership was expected of him in order to help in carrying on necessary conferences? In the most exclusive clubs in an old metropolitan center, such as Philadelphia, business connections counted for relatively little and conducting business at the club was frowned upon. This was no doubt less true in newer, smaller cities. Similarly, did an entrepreneur build an expensive house in a good neighborhood as an aid to economic success or because his wife wanted it? The use of correct, forceful English may have been more important than family connections. In short, to what extent was participation in exclusive social organizations or neighborhoods necessary to success, and to what extent was such participation a nearly inevitable result of it?

A minor criticism, again generic, is that local directories often list men whose chief activities were in fact elsewhere. Andrew Carnegie would be one such. Listed as a Pittsburgh steel man, he lived in New York and Europe. His participation in exclusive organizations in his city of early residence was small, but his acceptance by cabinet ministers and European aristocracy was extensive and often lucrative.

These cautions should not obscure the value of group studies such as this one nor contradict the

author's assumptions of considerable class stability but merely emphasize some limitations as to what can be learned and affirm the continuing utility of individual biography.

THOMAS C. COCHRAN
Eleutherian Mills-Hagley

WILLIAM R. JOHNSON. *Schooled Lawyers: A Study in the Clash of Professional Cultures*. New York: New York University Press. 1978. Pp. xvii, 215. \$15.00.

William R. Johnson's book on the history of legal education began as a dissertation on the University of Wisconsin Law School for the years 1868 to 1930. That study still forms the core of a work that examines the transition from frontier to "scholastic" conditions in professional training. In its present form, Johnson's work raises questions that extend far beyond the bounds of the local institution or the particular state.

The local picture remains important: a legal profession regulated by the "conviviality" of the circuit bar through perhaps the 1860s; a university interested in acquiring professional schools to protect its own position, even while the bar had little interest in school education; the transitional work of Dean Edwin E. Bryant, building the Wisconsin Law School as a lecture hall with law-office atmosphere (1889-1903); the university's deliberate choice to import the case method (1894) and Harvard product Harry S. Richards (1903); and Dean Richards's long hostility to the more traditional Marquette law school (established 1908).

One theme runs throughout: the mutual autonomy of a "natural" legal profession and a more aggressive educational bureaucracy. The university recruited elitist law professors who had little contact with practice. The working profession only belatedly accepted this design in the 1920s, because it then wished to fight competition, not just from immigrants or hustling young lawyers, but from banks and realtors that handled business for individuals.

Johnson considers but largely rejects the idea that the case method implemented a new corporate orientation among lawyers. This conclusion may be open to argument. It depends on his view that advocacy long remained more important than counseling businessmen as the characteristic work of Wisconsin lawyers. For this to be fully convincing, we would need concrete data on how late the lawyers who argued in court were also the prosperous or influential ones. Similarly, the case method may well have been scholastic, not modern or corporate, but someone should analyze for us the evidence about specific implementations of the method, as in Wisconsin. Johnson may rely too

much on dicta and sallies that some writers have offered about the method, when too few have written out their arguments for others to consider.

The problems that arise about John Dewey and progressive education arise here, too. Any argument that Dewey transmitted some new corporate ideology into education must rest on a close philosophical analysis of what his texts imply about society—not on any allegation that he nursed guilty or conspiratorial motives. Arguments about the content of progressive education should rest on demonstrations about the specific ideas that teachers implanted while they thought they only encouraged inductive learning. The same applies to any idea that the case method presented narrow "doctrines," in contrast to the "principles" conveyed by earlier lecturers. We need to be shown just what doctrines and what principles were entailed in each situation, and we need precise distinction between principles and doctrines. For the case method, we need also to be given social and ideological analyses of the people who sponsored the method. With exactly which of their social assumptions was the method connected? Into what context should we place the mind of someone like Richards, aggressively satisfied to work within the case method?

Schooled Lawyers does an excellent job of raising these questions, which are ultimately those about the relations between cultural institutions and corporate capitalism. Secure answers will depend on a cooperation between conceptual history and the new social history.

DANIEL H. CALHOUN
*University of California,
Davis*

PETER R. DECKER. *Fortunes and Failures: White-Collar Mobility in Nineteenth-Century San Francisco*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1978. Pp. xii, 336. \$20.00.

Peter R. Decker has succeeded in bringing the human element to the New Urban History. *Fortunes and Failures* manages to avoid the quagmire of relentless tables and statistical formulas by judiciously using both quantitative and qualitative data. Decker's account thus comes alive through the dreams and successes of men such as Levi Strauss and the crushed aspirations and anxieties of countless other merchants who could not stay abreast of change in this dynamic community.

Decker has actually written two books about San Francisco. The first examines the changing social and economic forces that accompanied the city's transformation from a commercial port city of 30,000 in 1852 to an industrial metropolis of

200,000 in 1880; the second analyzes the occupational mobility patterns of native- and foreign-born merchants during the same period. His success in analyzing the failure of the merchant elite to dominate San Francisco society overshadows the weaknesses one may find in his mobility analysis and illustrates the difficulty of integrating the two historical approaches.

From 1850 through 1880 the San Francisco merchants waged a futile battle against devastating fires, recurring periods of boom and bust, corrupt government, lawlessness, and the perceived threats by an organized class of city workingmen. Aggressive actions of the merchant class through various associations and the Vigilance Committees of 1851, 1856, and 1880 enabled them to dominate San Francisco society for a time. Their inability to control an erratic economy, however, and their resistance to the transformation from a commercial to an industrial society doomed them. By 1880 the commercial elite in San Francisco had given way to an industrial one.

Decker's account of white-collar merchant mobility is an important part of this study. The merchants, after all, controlled and largely created San Francisco society. The results of his analysis follow the now-familiar nineteenth-century American pattern: low rates of residential persistence; greater lifetime occupational mobility for white-collar workers than for laborers; the development of residentially segregated neighborhoods (the wealthy in the hills; the poor in the valleys). There are also some surprises: merchants, subject to erratic business cycles, frequently experienced downward mobility and often bankruptcy; blue-collar workers persisted in the community longer than white-collar merchants; and German-born merchants experienced greater rates of upward mobility and residential stability than their native-born counterparts.

The mobility analyses are, however, somewhat less satisfying than Decker's excellent account of social change in San Francisco. As one examines the mobility and persistence data, several questions arise. First, the miniscule size of certain samples employed in this study try one's confidence. For example, the author's sample for San Francisco workers in 1852 and 1880 contain only 669 and 731 individuals respectively. Moreover, we are given no indication that tests of statistical significance were conducted to determine their level of validity. When the data is divided by occupational levels, the problem is magnified. A sample of thirty workers, for example, is used to generalize about low blue-collar mobility in 1850-1880 San Francisco.

Second, as mentioned, the mobility rates of German merchants exceeded those of native-born

merchants throughout the period. More than half, according to Decker, were German-Jews with prior merchant experience. Yet at no point are the mobility rates of Germans and German-Jews separated. Several previous studies, including Thomas Kessner's *The Golden Door*, suggest that a combination of first and last names provides a relatively accurate means of separating Jews from others in the population. The success of the German merchants makes this differentiation imperative.

Third, while the occupational divisions employed in this study are widely known and accepted, they may be misleading as a surrogate for status at the white-collar level. It is likely that many merchants—particularly those resisting the change Decker so thoroughly analyzes—retained the same occupations while increasing their income dramatically. Successful merchants may have invested surplus cash in real estate, cable cars, or other public and private areas, rather than expanding their businesses or turning toward manufacturing. The use of income data would add an important dimension to this study.

Peter Decker has made an important first attempt to combine both statistical and traditional data in studying class differences in mobility rates. These differences, he argues, "are too often described only through statistical measures and techniques. Rarely are they explained within the social context in which they occur" (p. 250). This context, he suggests, includes the hopes and anxieties of those whose lives are being measured. It also includes the changing social forces in the community in which these workers reside. *Fortunes and Failures* creatively provides both the measurements and the social context. In doing so, it tells us much about San Francisco society and its merchant elite.

MICHAEL P. WEBER
Carnegie-Mellon University

JAMES P. WALSH, editor. *The San Francisco Irish, 1850-1976*. San Francisco: The Irish Literary and Historical Society. 1978. Pp. 150. \$15.00.

This slender volume is a by-product of the activities of the Irish Literary and Historical Society of San Francisco, which has encouraged Irish studies since 1945. Most of its nine essays portray phases in the lives of some prominent San Franciscans of Irish ancestry during the past one hundred years, but two articles, by James P. Walsh and George A. Colburn, somehow involve Jewish politician Abraham Ruef, who at the turn of the century masterminded the city, and Irish zealot Father Charles E. Coughlin, who during the Great Depression harangued the country from a Detroit parish. A preface by Walsh as editor and a brief introduction by

Moses Rischin sketch the historiography of Irish immigration and place the ethnic group within the larger context of immigration to the United States.

The sudden rise of San Francisco provided unique opportunities for an early flowering of a particular political genius of the Irish, and their contributions to municipal affairs and state politics stand out in the essays. With the vigilance committee of 1856 as background, Walsh draws a vignette of the first Irish master politician in California, David C. Broderick. William A. Bullough measures the transitional role of Christopher Augustine Buckley, the city's Democratic saloon boss during the 1880s, against some of the cultural aspirations of subsequent reform politicians. The survival of Progressivism during the 1920s is stressed by Roger W. Lotchin in his article on the lawyer John Francis Neylan, who shaped the editorial policy of the Hearst organization in favor of municipal ownership of the water and transportation systems. Kevin Starr's essay, presenting Governor Jerry Brown as Zen Jesuit, demonstrates the survival of the Irish gift for practical politics.

The discussion of political activities frequently turns with specific attention to the Catholic Church and the law and to journalism, education, and scholarship in the lives of these San Francisco Irish. A third contribution by Walsh discusses Peter C. Yorke's perfunctory service as a regent of the University of California in the light of the priest's criticism of 1899 that Irish-Americans were not adequately represented in the student body and faculty of the state university. The outstanding work of quite a different regent of Irish ancestry, Garret W. McEnerney, is related by John Riordan in an essay detailing the successes of the eminent lawyer. Seamus Breatnach surveys the contribution of the San Francisco Irish to scholarship, emphasizing the achievements of Robert I. Burns, John P. Diggins, and Emmet Larkin.

A haunting photograph of Seamus Moriarty, poet, miner, and gardener in Golden Gate Park, evokes the image of the vast number of people unrepresented in the book. Their story has yet to be told. The articles lack concept and cohesion as a collection which could have accounted for the San Francisco Irish; references to the "Irish subculture" or the "Irish-Catholic San Francisco mind" stimulate one's interest with the immigrant group, but they do not enlighten. Although the articles on Yorke, Ruef, and Brown are reprints, prominent protagonists, such as Kate Kennedy or Denis Kearney, receive no space, and Kearney's name is misspelled the few times it crops up. When an article deals only with a segment of a man's life, ambiguity arises, as in the case of Neylan, the "San Francisco Irish Progressive," who as a regent during the loyalty oath fight at the University of

California in 1950 was an ardent admirer of Senator Joseph R. McCarthy. Evidently the printer had difficulties with the book. One essay, set in larger type than the other contributions, accentuates the small print of the book, and all articles have their share of typographical errors. Despite these strictures, the publication will be welcomed as a contribution toward understanding the complexities of ethnic life in San Francisco.

GUNTHER BARTH
University of California,
Berkeley

GEORGE J. PRPIC. *South Slavic Immigration in America*. (The Immigrant Heritage of America Series.) Boston: Twayne. 1978. Pp. 302. \$11.95.

The appearance of *South Slavic Immigration in America* by George J. Prpic, a part of Twayne's Immigrant Heritage of America Series, promises much to both the general public and the professional historian for a number of reasons. Above all, it deals with a rather sizable portion of our population, Americans from southeastern Europe, about whom we remain grievously ignorant. While the few earlier works in English were either sketchy or profiles of single groups, Prpic is obviously well equipped by his previous publications to write an inclusive outline of South Slavic immigration. Also, the introductory statements by both the series editor and the author himself of his interest in family and women's history imply that the work will cover new ground. On the whole, unfortunately, these expectations are not realized.

Admittedly, Prpic has assumed a difficult task—a historical survey of a highly fragmented population from a still little-known part of the world. The plethora of groups, both ethnic and regional, their frequent conflicts, the rather involved and complex sociological relationships, and the continued paucity of published accounts all make his effort a courageous attempt. These problems and others, nevertheless, overwhelm the author, who presents a dated and somewhat confusing overview of the South Slavs.

Most troublesome is the work's structure. The book consists of twenty-one chapters of varying length, from six to twenty pages, which occasionally incorporate multiple topics not integrated by the titles. A historical continuity does exist, but the coverage is highly selective. Insufficient attention, for example, is given to immigrant motivation, living and working conditions, and the social history of the groups in general. While it is true that prior research on these topics is still fragmentary, Prpic ignores the rich folkloric and anthropological literature on his subject. What remains, then, is

old-fashioned institutional history, chiefly political activity as well as recent religious disputes which have rent American Orthodoxy.

In addition, professional scholars will take issue with a number of the author's specific comments and interpretations: our knowledge of the South Slavs did not begin with Emily Greene Balch's excellent work (p. 20); John R. Commons is certainly no authority on Slavic immigrant adjustment (p. 81); most Balkan workers were not unionized before 1920 (pp. 89, 157); Joe Magarac, the manufactured "folk" hero came from a non-Slavic originator, not Croatian folklore (pp. 131-132); and while we are grateful for his inclusion of Bulgarians, Macedonian ethnic consciousness in this country is now independent of those ties, and so it probably will last longer than that of Serbians, Slovenians, and Croats (p. 254).

It is clear that, in this age of widespread interest in familial antecedents, the author and publisher oriented the work more toward the general public than the scholarly community. Yet, even with that consideration, the work is deficient. Especially regrettable are the absence of any maps of America or southeastern Europe, a real failing in the case of the latter. Further, the index of four pages is barely adequate. While the study is well documented and an occasional photograph offers a glimpse of the fabric of South Slav American social life, it is not likely that the work will appeal to many non-scholarly readers.

VICTOR R. GREENE
University of Wisconsin-Milwaukee

JÓZEF MIĄSO. *The History of the Education of Polish Immigrants in the United States*. Translated by LUDWIK KRZYŻANOWSKI. (Library of Polish Studies, number 6.) New York: Kosciuszko Foundation. 1977. Pp. 294. \$8.95.

The stated purpose of *The History of the Education of Polish Immigrants in the United States* is "an attempt to show the beginnings, development, and functioning of the Polish schools in America" (p. 12). Consequently, Józef Miąso limits his work primarily to the dual areas of curricular growth and ethnic parochial schools. He argues that education among Polish-Americans grew slowly because of a combination of the nonliterate traditions of the Polish peasantry and the economic realities requiring immigrant children to enter the labor market at an early age. Furthermore, those who wished to preserve their Polish heritage were faced with a choice between public schools offering no Polish studies or parochial schools catering to these cultural needs but charging a tuition which strained family finances.

Miąso maintains that early parochial schools differed little from elementary schools in partitioned Poland. Science and physical education were completely neglected while the level of instruction in the English language and American history remained low. Teaching methods seldom progressed beyond rote memorization with no attempt to stimulate thought. Despite these criticisms, however, Miąso feels that parochial education did serve a vital function as a central sociocultural institution for the maintenance of community solidarity. Miąso maintains that such schools saved Polish-American youth from illiteracy and "denationalization," yet beyond the confines of the ethnic community such an education was of limited use.

The author views the end of World War I as a dividing point after which Polish-Americans began to orient themselves less toward Poland and more toward their American environment. The depression sparked an exodus of students from parochial to secular schools that helped to fracture the bonds of ethnic isolation.

Miąso's book is well conceived and rests upon a firm foundation of archival research. It is, however, somewhat limited due to its concentration on curricula and parochial schools. A further limitation is its age. Since its first publication in Poland in 1970 several other sources have become available. Researchers in the field would do well to complement Miąso by reference to dissertations by Ladislas Siekaniec ("The Polish Contribution to Early American Education, 1608-1865" [1976]), who deals in more detail with the antebellum period, and William J. Galush ("Forming Polonia: A Study of Four Polish-American Communities, 1890-1940," University of Minnesota, 1975), who deals with the relationship of education to immigrant mobility during the period of the New Immigration.

JAMES S. PULA
University of Maryland

CAROLINE GOLAB. *Immigrant Destinations*. Philadelphia: Temple University Press. 1977. Pp. x, 246. \$15.00.

Caroline Golab has written an insightful work on the motives and settlement patterns of Polish immigrants. In the process she offers much information on other newcomers as well. Her locale is Philadelphia between 1880 and 1920, a time of large-scale immigration from eastern and southern Europe to that port city.

The author argues that Polish immigration was essentially economic. Most migrants were peasants from an overcrowded land, young men eager to earn and then return enriched to their old vil-

lages. Within an "Atlantic economy," they were migrant laborers who sought maximum income and came to America by choice. Their settlement pattern in Philadelphia reflected their goals: twelve enclaves shared with natives and other new immigrants adjacent to their workplaces.

Yet Philadelphia was not an appropriate destination for Poles. The city was large but old, with many medium and small industries requiring specialized skills. The young peasants had muscle and will, but most could do only unskilled labor. The simultaneous arrival of thousands of Italians and east European Jews made the Polish situation worse. Like the Slavs, the Italians were typically young men of agrarian background seeking temporary employment. Through a *padrone* system of labor contractors many jobs requiring unskilled labor came under Italian control. Ethnic stereotypes favored Poles in the local steel industry, but the modest number of such jobs led most to seek work in other localities. Transience characterized the community, with perhaps one out of five persons leaving each year. Somewhat more stable were professionals and petty entrepreneurs, who often led the parishes and ran the few ethnic businesses.

The author has done extensive research and presents her material with clarity and style. Charts and maps summarize information drawn from city directories and parish and official records. Comparisons with Italian and Jewish patterns of mobility and job holding give a wider scope to the work. The immigrants emerge as persons of purpose and calculation, with the Poles and Italians firmly oriented towards a return to home abroad.

The work raises some questions unperceived by Golab. Could the numerous but small settlements have adversely affected occupational mobility for ambitious Poles? Polish-speaking Jews seem to have perpetuated a commercial dominance which declined in other, more concentrated colonies. The slogan *Swój do swego* ("Buy from your own") in advertisements in the ethnic press across the nation was one symbol of a drive for community. A basic community institution was the mutual aid society, which had an important economic function. Even small settlements would have one or more, and the effect of membership on residential stability in such a transient group would merit investigation. One other minor point is the perpetuation of the myth of the leadership role of the priest. The author passes over recent work by Victor Greene and me noting the presence of lay initiative in parish and society and relies instead on filiopietistic biographies of clerics.

The book has great merit and should be read by all scholars concerned with immigration.

WILLIAM J. GALUSH
Loyola University, Chicago

HAROLD X. CONNOLLY. *A Ghetto Grows in Brooklyn*. New York: New York University Press. 1977. Pp. xv, 248. \$15.00.

A Ghetto Grows in Brooklyn is a chronicle of three hundred years of the black presence in Brooklyn, New York. Almost as much space is given to the post-1940 period as to the earlier three centuries. The book fails to meet the general standards of black community historiography established in the last fifteen years by studies of New York, Harlem, Chicago, Detroit, New Orleans, and Cleveland. Instead it consists mostly of short, impressionistic, topical essays. The 186 pages of text (excluding notes and maps) are divided into eleven chapters, a conclusion, and an epilogue, which are further divided into fifty-eight sub-headings within chapters. Essentially mini-essays, the topics in the pre-1940 period focus on slavery, prominent blacks, churches, schools, and so forth. The post-1940 narrative focuses on the Bedford-Stuyvesant area, its demographic changes, election of black officials, and problems in employment, housing, health care, and education. Harold X. Connolly concludes that the failure of federal programs in Bedford-Stuyvesant "suggests that neither progress nor even stability has come to urban blacks during the past three-quarters of a century" (p. 228).

Connolly must be credited with tackling a difficult topic; New York City generally, and Brooklyn in particular, have been avoided as areas of inquiry by historians. Problems of size, complexity, and diversity have led historians to seek more manageable topics. Connolly's weakness, however, lies precisely in his failure to deal with Brooklyn—its size, complexity, and diversity; he has not solved the problem of how to study blacks in Brooklyn. What is the proper frame of reference—neighborhood or borough or city? What is the relationship of urban sectors to the whole? What kinds of local autonomy exist and how important are they? How does one examine local business or employment or institutions in a city with relatively inexpensive and accessible mass transportation?

Connolly's discussion of the black community itself is also weak. It is a record of leaders and the founding of institutions with scant analysis. Nowhere does he provide an adequate overview of the community—its institutions, structure, issues, ideology, or life style. There are no discussions of the dynamics of class or ideology or how the community dealt with change, in-migration, or growth. And most surprisingly, the long discussion on contemporary black politics ignores the well-publicized factional struggles among Brooklyn blacks.

Overall, this is local history, erratically chronicled.

DAVID M. KATZMAN
University of Kansas

THOMAS LEE PHILPOTT. *The Slum and the Ghetto: Neighborhood Deterioration and Middle-Class Reform, Chicago, 1880-1930*. (The Urban Life in America Series.) New York: Oxford University Press. 1978. Pp. xxiv, 428. \$17.95.

Chicago's tenements have never lacked publicity. Low-income families in dilapidated dwellings were featured in *Hull-House Maps and Papers*, Robert Hunter's *Tenement Conditions in Chicago*, Louis Wirth's *The Ghetto*, Edith Abbott's *The Tenements of Chicago*, and St. Clair Drake and Horace Cayton's *Black Metropolis*. More recently, Allan Spear and William Tuttle, Jr., probed the consequences of black confinement, while others have looked at immigrant mobility. There are comparable monographs on the immigrant or black experience in Boston, Cleveland, Detroit, Milwaukee, Philadelphia, and New York. In all these places, blacks faced special barriers while immigrants were relatively free to move upward and outward as their perseverance, skill, and money dictated. "The color line," noted Abbott in 1936, "as it appears in the problem of Chicago and of every other northern city, is too important to be overlooked."

Thomas Lee Philpott takes a long, hard look at Chicago's color line in housing. His impressive study of neighborhood deterioration and housing reform from George Pullman's day to the New Deal juxtaposes the immigrant and black experiences and explores the private views of reformers on the "colored question." His evidence is overwhelming, his conclusion sobering. "To keep blacks at a distance socially, Chicagoans segregated them spatially" (p. 119). Whatever their other differences, foreign and native-born, Jews, Catholics and Protestants, Democrats and Republicans, rich and poor, conservatives and reformers were partners in this united white front. *The Slum and the Ghetto* is a powerful riposte to those who wonder why blacks failed to emulate the immigrant trek from tenements to suburbs in two generations.

Ethnic clustering in the tenement district, Philpott argues, was temporary and voluntary. No group monopolized a neighborhood, there was steady turnover, and every group had members living outside the area. Blacks, less than 2 percent of the population in 1900 and nearly 7 percent by 1930, occupied a long corridor bounded by railroad tracks and an elevated line. After the 1919 riot, they took over contiguous neighborhoods vacated by white flight. Black Belt residents paid higher rents for worse quarters than did immigrants; they took in more boarders and had to tolerate the city's vice district. Intimidation and violence kept nine-tenths of the Negroes in areas over 80 percent black. "No immigrant group was, or ever had been, so impacted" (p. 142).

Social reformers and civic-minded businessmen tackled the housing problem with conferences and surveys, a Municipal Lodging House, tenement ordinances, and a variety of organizations. They were impotent in the face of white property owners' associations and the segregationists' born-again discovery of restrictive covenants. In the post-riot decade, however, philanthropists built the first large housing projects, two for whites and one in the Black Belt. Not all reformers thought low-income housing need yield a profit, but they agreed that reconstruction and increased social services were the best they could do for blacks.

The author's righteous stance for integration and public housing results in harsh judgments of reformers who failed to see the light in the years between 1880 and 1930. Thus, Julius Rosenwald and financiers behind the housing projects are guilty of "privatism and racism" (p. 205), while Jane Addams and Mary McDowell are faulted because "they did not advocate integration" (p. 299). Unfortunately, this carefully researched book, which bristles with lot sizes, room dimensions, and rent structures, does not systematically examine ownership or profit margins in either immigrant or black tenements. Yet Philpott sustains his important thesis. *The Slum and the Ghetto* should be required reading for policy makers today.

LOUISE C. WADE
University of Oregon

GORDON W. KIRK, JR. *The Promise of American Life: Social Mobility in a Nineteenth-Century Immigrant Community, Holland, Michigan, 1847-1894*. (Memoirs of the American Philosophical Society, number 124.) Philadelphia: American Philosophical Society. 1978. Pp. 164. Paper \$8.00.

Gordon W. Kirk, Jr.'s *The Promise of American Life* is, I suspect, one of the last of a breed of monographs which have dealt with social mobility at the community level. Some years ago a prominent sociologist made the pronouncement that the topic no longer held much interest for his discipline. If the current trends in historiography are any guide, historians have also grown weary of it.

To be fair to Kirk, out of the half dozen or so monographs which have been published on social mobility, his effort is a good deal more satisfactory than most. He follows a familiar path with chapters on change in the occupational and economic structure of Holland, and with intra- and inter-generational mobility. The results, carefully integrated into both the sociological and historical literature, show that the frontier experience had greater influence upon rates of mobility than did either urbanization or industrialization. As a work of historical sociology, which is what it is intended

to be, the book is a solid contribution. However, its long publication delay precludes citations later than 1974 and the similar use of multi-variate statistical techniques, which are now widely available.

As a work of history, unfortunately, *The Promise of American Life* has all the faults that have from time to time been found with this type of study. It is narrow in scope, written in a flat, awkward, pedestrian style, and because of the author's concern with technique, the narrative is drained of almost everything else of potential interest. The reader cries out for more information about the people who lived in the town and on the farm and about Holland's institutions and politics, business, and industrial life.

As Kirk points out, Holland is representative of a large number of ethnically homogeneous and rurally oriented communities in the midwest. It would seem therefore that such a community deserves a monograph combining fine-tuned detail and extensive documentation with extensive data culled from census manuscripts.

The Promise of American Life is a product of the early years of the historian's brush with social science methodology. Since then a number of community studies have appeared (for instance those by Dawley, Bodnar, and Doyle) that place less stress on the mechanics of change in the social structure of communities and more on the effect these changes made on ideology, community cohesiveness, and working conditions. To be sure, historians should publish work that is methodologically oriented, but an article in a specialist journal is a more suitable vehicle than an monograph.

MARK FRIEDBERGER
Newberry Library

THOMAS MONROE PITKIN and FRANCESCO CORDASCO. *The Black Hand: A Chapter in Ethnic Crime*. Totowa, N.J.: Littlefield, Adams and Company. 1977. Pp. 274. \$4.95.

Mystification continues to be the major response to criminal activity that has foreign roots. Americans are perhaps most familiar with the Mafia, usually described as a sinister and secret international terrorist organization with world-wide links. This volume is a study of the activities of *La Mano Nera*, which began as an extortionist organization in New York early in the century. Whatever the terms used to describe such criminal organizations, they all followed the code of *omertà*, refusing to divulge their operations and instilling such fear in their victims that the police seemed powerless to control their activities. America's Italian neighborhoods were victimized by variants of Calabria's older *Onorata Società*, the Neapolitan *Camorra*, and the Black Hand.

The authors do a workmanlike job of describing the operations of the Black Hand. There are other considerations, however, that also ought to capture the attention of the historian of crime. The new psychohistory offers us invaluable insights about the unconscious, often disguised, emotional fall-out and psychological deficits which developed in the lives of America's foreign criminals. But immigration specialists still want to go over and over such subjects as social mobility, the role of politicians and the police, immigration legislation, educational deprivation, and financial disabilities. Instead, one needs analysis of the motivations (especially psychiatric ones) of the members of ethnic groups who became criminals. In other words, we ought to move beyond material considerations toward the scrutiny of feelings and moods that remain ignored but that influence human behavior massively.

The authors make the point that the Black Hand had a life of its own, that it was unencumbered by the legacies of the Neapolitan *Camorra* or Sicilian Mafia. Perhaps this was so at the inter-personal level, but intrapsychically the first and second generation hoods who oozed upward from America's ghetto streets bear many of the same characteristics.

This book makes its most important contribution as a study of the indigenous origins of a particular branch of organized crime. It also serves as a reminder that the vast majority of American Italians remained law-abiding citizens who sought to be clear of the stigma of criminality. (As early as 1907 Chicago Italians organized a White Hand Society to fight the Black Hand.) Americans still find it hard to believe that crime could have been encouraged here at home in their corruptly governed cities, filled with temptations for amassing illegal profit. Thus, the alien nature of a Mafia mythology continues to satisfy the country's naive need for a scapegoat, fulfilling the wish that sin has come only from abroad.

ANDREW ROLLE
Occidental College

DANIEL T. RODGERS. *The Work Ethic in Industrial America, 1850-1920*. Chicago: University of Chicago Press. 1978. Pp. xv, 300. \$15.00.

The work ethic has occupied a large place in American thought, although, until recently, historians have tended to slight its importance in favor of exploring such related subjects as success and mobility. Perhaps the very pervasiveness of the work ethic has been responsible for this oversight, convincing us that our feelings about work are not susceptible to historical analysis—that work and its compulsions have always been with us in more

or less the same form, and that they will always remain so. Happily, as Daniel T. Rodgers's monograph on the work ethic in industrial America demonstrates, this is a subject well worth exploring. Rodgers sets his book between the years 1850 and 1920, roughly the period in which industrialization transformed American life to the point where an economy of general abundance (and excessive consumerism) became possible. This was a period, he argues, when the inherent contradictions between various elements of the work ethic were exacerbated by rapidly changing labor conditions. The result was an erosion of the ascetic character of the work ethic and a gradual turn toward the "noisy gospel of play." A second characteristic of writing about work in this time was the growing tendency to attach a generalized and vague belief in the moral importance of work to various popular causes: women's rights, industrial unionism, and various progressive causes. Rodgers contends that the effect of these changes was to consign the work ethic to a less central place in the hierarchy of American values.

In the first of three sections, the author touches on a variety of labor-related movements, describing how they used the work ethic as an explanation and justification for their programs. These included the cooperative movement, industrial democracy, and profit-sharing plans. Their failure, together with criticisms directed by a variety of reformers against mechanization, led to a growing belief that leisure should be the source of happiness and fulfillment. In his subsequent section on play and repose, Rodgers suggests that doubt—and perhaps realism about the nature of industrial laboring conditions—persuaded middle-class writers to develop a new social literature advising rest, relaxation, and exercise as antidotes to alienating work. A final section on industrial wage earners, women, and politicians is a critical discussion of the uses and misuses of the work ethic in discussions of political and social change around the turn of the century. Thus, he concludes, as the relationship of individuals to their labor changed, the work ethic became a more confusing, abstract, and irrelevant part of the arguments that raged during the period. In a sense this picture of a diminished role of the work ethic is true, and Rodgers makes an interesting case for it. But, while the intrusion of the work ethic into some discussions may not have been particularly fruitful, it remains true that the work ethic and ideas related to it were at the center of the great Progressive debate over the nature of the modern industrial order.

JAMES B. GILBERT
University of Maryland

RICHARD P. HORWITZ. *Anthropology toward History: Culture and Work in a 19th-Century Maine Town*. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xiii, 197. \$14.00.

According to anthropologist Anthony F. C. Wallace, quoted on the book jacket, this study of occupational categories in Winthrop, Maine between 1820 and 1850 "represents a major innovation combining both rigorous historiography with the conceptual apparatus of ethnohistory as developed in recent years by cultural anthropology." Despite this large claim, historians will be disappointed with Richard P. Horwitz's flawed but interesting experiment.

Although the jargon and level of abstraction are excessive at times, the author's presentation of the conceptual apparatus and his own approach to the case study in chapters one through three are not the problem. Historians can benefit from reading his argument for semantic analysis and, specifically, for systematic investigation of the rules by which groups of nineteenth-century Americans "catalogued" themselves. The relation between Horwitz's methodological prescriptions and his historical description is less clear, but he seems to have read his sources carefully. His analysis of how major occupational groups—farmers, farm help, mechanics, workingmen, gentlemen, businessmen, and professional men—are characterized in newspapers, sermons, local histories, memoirs, and so on offers few surprises to anyone familiar with such characterization in other antebellum communities.

What must surprise any social historian is Horwitz's failure to discuss the limitations of his presumption that these sources will provide a sufficient picture of how Winthrop's residents—regardless of the nongeographical groups to which they belonged—characterized their own and others' occupations. He never asks, for example, whether those who contributed articles or letters to his most heavily used source, *The Maine Farmer*, represented the perspectives of all major categories of agricultural workers. His own evidence suggests they do not. He illustrates his "paradigm" of the farmers' world with an 1833 address which classified subsistence farmers with "part-time farmers" because, the speaker "accused, idleness was their other pursuit" (p. 45). Horwitz merely remarks that "residents apparently tolerated quite a bit of variation in the use of the label." He never asks how subsistence farmers may have categorized themselves. Similarly, his chapter on farm help claims "common notions about their character" based largely on unflattering descriptions in *The Maine Farmer* (p. 56). The general bias of Horwitz's sources toward the limited range of respectable

opinion has some exceptions, such as the complaints of a few factory girls about depictions of their motives. But he does not go beyond such conflicts of perspective within his sources to ask whether and why some perspectives may remain unrepresented. He simply presumes, uncritically, that the town was the most important social and cultural unit for all of its residents and so constituted their dominant "communicative community" (p. 29).

Horwitz believes that "a study which combines the qualitative analysis of cognition and the statistical analysis of behavior would achieve better results than either analysis alone" (p. 132). His own book would have been improved by pursuing that combination at least to the extent of providing quantitative checks on measurable generalizations, especially those which seem closer to myth than reality. The relative size, prosperity (as measured by real property), and other traits of his different occupational groups could have been determined so easily through the manuscript census of 1850 for this town with a total population of only 2,154.

Horwitz has provided in chapters four through nine a useful survey and much enjoyable illustration of certain antebellum perspectives on major occupational groups. However, this attempt at empirical demonstration of the usefulness of ethnohistory for historians offers too few fresh insights and too uncritical an approach to validation of old ones to win many converts to the elaborate methodology.

CLYDE GRIFFEN
Vassar College

HENRY D. SHAPIRO. *Appalachia on Our Mind: The Southern Mountains and Mountaineers in the American Consciousness, 1670-1920*. Chapel Hill: University of North Carolina Press. 1978. Pp. xxi, 376. \$16.00.

In the next twenty years many readers will check Henry Shapiro's book out of libraries expecting to find detail regarding the land and the people of Appalachia at the turn of the century. They will be disappointed. This is not an investigation of a region but rather of the idea of a region. The subject is not Appalachia but *Appalachia on our mind*. The intellectual history that results is first-rate if, in places, rather ponderous and hard to understand with one reading. As might be expected since the real subject is the American consciousness, we learn much about national character, national purpose, regionalism, economic development, philanthropy, literary style, and the meaning of progress.

What made the Southern Appalachian mountains and mountaineers an important concern of the American mind between 1870 and 1920 was

what Shapiro calls the area's "otherness." This was, we are told repeatedly, "a strange land inhabited by a peculiar people." The mountain whites (the book is not about black people in the region) were, by the standards of the rest of the nation, old-fashioned, behind the times, and unprogressive. The next question, and the tension around which most of the book turns, is whether this otherness was good or bad. Local-color writers, who discovered Appalachia's difference after the Civil War, felt the place quaint and picturesque but disturbingly backward. The mountain girls of Mary Noailles Murfree's short stories were not permitted to marry the "outsiders" who loved them. The Protestant churches of the late nineteenth century did not even find quaintness in Appalachia. In the eyes of the missionaries, the mountain people were squalid, degenerate, and badly in need of salvation. This idea, Shapiro points out, was self-serving in that it created a vacuum for the churches to fill. Through faith the savages could be civilized and lifted into the blessings of modern American life. The same perspective colored the vision of those who would lift the mountain South economically. The Tennessee Valley Authority, although not within the chronological limits of the book, represents the culmination of the efforts to redeem Appalachia.

Others were not sure about either the degeneracy of the Appalachian people or the beneficence of modern America. William Goodell Frost, president after 1893 of Berea College in Kentucky and the subject of a Shapiro chapter, pioneered the viewpoint that different was not necessarily disadvantaged. Frost saw the mountaineers as heirs to the strengths and values of pioneers. He understood the potential asset to a nation that was becoming increasingly urban, industrialized, and peopled by immigrants. And Frost went further to question the idea of a homogeneous "national" civilization and character. What was wrong, Frost wondered, with diversity? Why not accept, cherish, and develop the difference of the mountain people, if that difference meant an exciting native tradition of crafts and folk culture and a close-to-nature lifestyle that some might find a desirable alternative to that in city slums?

Shapiro's final two chapters concern the revival of handicrafts and folk singing in Appalachia. But as we should expect by this stage of the book, the author is hardly concerned with a listing of festivals and exhibits. His subject is what the discovery of a regionally oriented folk culture means for America's conception of itself as a nation and a society.

RODERICK NASH
*University of California,
Santa Barbara*

TENNANT S. MCWILLIAMS. *Hannis Taylor: The New Southerner as an American*. University: University of Alabama Press. 1978. Pp. xi, 163. \$11.75.

Hannis Taylor (1851–1922) was a Mobile lawyer, author of books on constitutional history and other legal treatises, U.S. minister to Madrid during the second Cleveland administration, a spread-eagle imperialist, candidate for Congress, a Republican convert and friend of Theodore Roosevelt, and finally a Washington attorney who served occasionally on diplomatic claims and arbitration commissions.

Son of a ne'er-do-well North Carolina businessman, Taylor worked his way into Mobile's, then Washington's elite. He was committed to industrial progress, efficiency in government, and the advancement of "Anglo-Saxon" imperial destiny. In virtually all aspects of his career, however, Taylor was ambitious, shifty, egotistical, and frustrated. His principal legal writings were derivative; parts were actually plagiarized. As minister in Madrid, he gradually came to favor intervention in the Cuban revolution and balked at Cleveland's policy of neutrality. Cleveland regarded him as a poor diplomat, although perhaps unjustly, for Taylor was able to perform valuable claims work for Americans. In 1898, sensing political capital in his early advocacy of war with Spain, he challenged his home district's Democratic congressman for the party's nomination. Although he adopted the free silver issue in order to appeal to farmers within the district above Mobile, he was rejected. A second forlorn challenge, followed by a pathetic attempt to secure the presidency of the University of Alabama, sent him to Washington, the Republican party, and shameless but seldom requited wooing of President Roosevelt. (Taylor once sent a moose head to the president; it was politely returned.)

Taylor was a model "New South" optimist and exemplar of Southern progressivism's preoccupation with race and governmental efficiency. He was unrepresentative of his region as an outward-looking urbanite who did not share rural southerners' suspicion of overseas expansion and as an advocate of a two-party South. Otherwise it is not entirely clear what the author means by the book's subtitle, "New Southerner as an American."

Professor McWilliams has lavished meticulous labor upon his not-very-important subject. There being no Hannis Taylor papers, he has in effect created a collection through his own correspondence with descendants and careful searching of Taylor's correspondents' papers. The organization of the book is sensible, the prose simple and clear. Taylor deserves only the eighty-nine pages of text

in this small volume. The text is almost overwhelmed by prodigious scholarly paraphernalia, however: forty pages of notes, thirty-one of bibliography, and an eleven-page index.

JACK TEMPLE KIRBY
Miami University,
Oxford, Ohio

JOHN A. JAMES. *Money and Capital Markets in Postbellum America*. Princeton: Princeton University Press. 1978. Pp. xv, 293. \$16.00.

John A. James's new book is a welcome addition to the growing body of scholarship on nineteenth-century capital markets. In addition to a discussion of the structure and performance of the American banking system and postbellum capital markets, James also offers new evidence on the decline of interregional interest rate differentials in the years before the Federal Reserve Act.

The first three chapters are devoted to a detailed examination of the nation's banks as they operated under the National Banking Acts of 1863 and 1864. The decline and subsequent resurgence of state banking is analyzed as well as the growing importance of demand deposits in the money supply. Chapter two focuses on the kinds of credit instruments available in the late nineteenth century and the fashion in which banks were able to manage their portfolio of earning assets.

As a prologue to his discussion of capital mobility, James examines the correspondent banking system. He describes the American banking system as an integrated whole, despite prohibitions against nationwide branching. "Rural banks," he contends, "were not isolated and unconnected entities; rather they were part of a widespread and intricate network linking city and country banks known as the correspondent banking system, which facilitated the flow of capital between regions" (p. 95).

Chapters five and six are more analytical, examining the fashion in which the interregional flow of funds took place and presenting an explanation for the decline in interregional interest rate differentials. The long-term capital flow was from east to west, that is, from areas with low interest rates to those with higher rates. Because of the "pyramiding" of reserves which characterized the national banking system and the tendency of country banks to maintain city balances, the flow of short-term capital was from west to east, or more specifically to the New York money market. The latter city's banks thus acted as gigantic conduits of loanable funds, collecting the inflow of short-term capital and diffusing these funds throughout the country. This reallocation process was accomplished by in-

terbank borrowing, direct interregional lending, eastern purchases of western bank stock, and the growth of the commercial paper market.

James devotes the final chapter to a consideration of converging interregional interest rates. He rejects the assertion of Lance Davis that the spread of the commercial paper market was responsible for a narrowing of interest rate differentials by pointing out that the temporal and geographic pattern of interest rate movements is not consistent with expansion of the commercial paper market. James looks more favorably upon Richard Sylla's contention that a decline in the monopoly power of national banks accounted for a large part of the decline in interest rate differentials. But he rejects Sylla's hypothesis that the lowering of capital requirements for national banks by the Gold Standard Act of 1900 was responsible for an increase in the number of banks and a decline in monopoly power. Instead, argues James, growth of state banks and, to a lesser extent, private banks increased competition and reduced interest rates.

James has produced a solid, professional work which makes up in usefulness what it may lack in style and imagination. For scholars who would understand the functioning of money and capital markets in the late nineteenth century this work is required reading.

DONALD R. ADAMS, JR.
Southern Illinois University,
Carbondale

GEORGE LISKA. *Career of Empire: America and Imperial Expansion over Land and Sea*. (Washington Center of Foreign Policy Research, School of Advanced International Studies, Johns Hopkins University.) Baltimore: Johns Hopkins University Press. 1978. Pp. xi, 360. \$17.95.

This volume, the latest in George Liska's growing list of analytical studies of America's relationship to a changing world, traces the career of the United States as an imperial nation. It establishes a framework for understanding the American experience in territorial, strategic, and economic expansion by sketching the rise and decline of both the Roman and British empires. These two precedents suggest that empires, in their exercise of power, require an original drive dictated by the quest for security—perhaps even survival—and subsidiary drives which explain why empires continue to expand after the original requirements have been achieved. The movement of Rome and Britain beyond their core empires, Liska explains, resulted largely from the desire either to ensure the security of the core or to satisfy the expansive dispositions of dependent peripheral actors. At

times the latter drive reflected internal pressures for continuing expansion to achieve domestic stability in the absence of external dangers. The resultant expansionist policies generally revealed an interplay between strategic and economic factors, between security requirements and sustenance needs. The decline of both the Roman and the British empires, Liska finds, resulted from disproportionately petty challenges that earlier they had withstood with apparent ease.

Quite reasonably, the author finds the initial external threat that led to an American imperial mentality in the overland encirclement by the French. British effort removed that danger by 1763. In achieving independence twenty years later, the United States failed to annex Canada, but in time Americans discovered that Canada was too small a threat to make the cost of separating that region from the British Empire worthwhile. Until the 1840s both the domestic and external conditions of the United States discouraged further territorial expansion. When the country at last completed its core empire with the annexation of Texas, Oregon, New Mexico, California, and Hawaii, it held an exalted position in the New World; it viewed its Spanish and Mexican antagonists with condescension. Still, continental expansion was in part the product of fear—worry over British, French, or Japanese designs on Texas, Oregon, California, and Hawaii. If the fears were exaggerated, the denial of these acquisitions to the United States would have altered the country's development considerably. Subsequent movements beyond the core empire, beginning with the Philippines in the late nineteenth century, resulted from concepts of manifest destiny, the revised Monroe Doctrine, the new principle of the Open Door, and, adds Liska, delusions. In explaining the growth of the American empire the author emphasizes the pressures for expansion; except in general terms he ignores the evolution of ends.

America's effort to manage the postwar reconstruction of Europe proved to be only the beginning of a new world role which ultimately assumed the form of a global defense structure and a worldwide informal empire of trade and investment. Expansion flowed partially from a perceived global threat centering in the USSR and partially from a desire to strengthen the postwar world economy through international commercial and monetary arrangements. In the process, the United States brought three traditionally resistant powers into the American system—Germany, Japan, and Britain. The American free trade empire culminated in the Trade Expansion Act of 1962 and the subsequent rounds of tariff reductions. The most conspicuous element, in the expansion of American trade and investment were the American-based

international corporations. In time the multinationals demonstrated the inability of American corporations to compete abroad. The mounting trade deficits of the 1970s levied exorbitant costs on American trade and defense policies. The Vietnam war broke the ascendancy of those who had maximized the nation's power and influence in world affairs—the imperial presidents, the unified military and bureaucratic leadership that organized and managed the imperial structure, and the political, scientific, and economic elite whose expertise had been essential for the imperial effort. As with Rome and Britain, a small war with its heavy costs sent imperial American power and influence into decline. Liska's syntax, if complex, does not impede the discovery of his laudable insights, but the book's detail, repetition, and digressions have lengthened it without elucidating its many important and exciting themes.

NORMAN A. GRAEBNER
University of Virginia

FREDERICK S. HARROD. *Manning the New Navy: The Development of a Modern Naval Enlisted Force, 1899–1940*. (Contributions in American History, number 68.) Westport, Conn.: Greenwood Press. 1978. Pp. xi, 276. \$18.95.

The most neglected of all American minorities, the Navy enlisted man, is beginning to get some belated historiographical attention. Frederick S. Harrod traces changes in policies relating to the Navy enlisted man during the first forty years of this century.

Spurred by the need for large numbers of "sailor-technicians" to man the modern fleet, the Navy began using public relations techniques to draw fresh manpower from the interior of the country instead of from the seacoast. This brought in a better class of men and made the service more truly representative of the nation as a whole. To prepare these men for their duties, special schools and correspondence courses were established, making the Navy "a major educational establishment" (p. 167). With bigger ships and more space the Navy was able to introduce such comforts as bunks, lockers, laundries, dishwashers, as well as improved lighting, ventilation, and messing arrangements. Sports were emphasized as a healthy and appropriate form of recreation. Modest pay increases, bonuses, new ratings, and a clothing allowance made Navy life more attractive. The military justice system was modernized, and there was a tendency toward clemency for first offenders. As a result of these changes, writes Harrod, by the 1920s and 1930s the Navy "had developed a career enlisted force for the first time in its

history: reenlistment rates were high, desertion rates were low, and the service enjoyed a sizeable core of skilled manpower" (p. 168). Moreover, the men identified themselves with the Navy and not with seamen. As a part of the effort to make the fleet more homogenous, aliens were barred after 1907. Blacks saw their opportunities steadily diminish, and they were not enlisted at all between 1919 and 1932. After 1932 they were limited to servant billets.

Harrod has done impressive research in official and private records, both manuscript and printed, including service magazines and some newspapers, in addition to dissertations, books, and articles. The lack of access to personnel files, and the relatively small number of reminiscences by enlisted men of necessity makes this mainly a study of administrative changes as seen from the top. It is nonetheless an important and well-written book.

Harrod tends to discount the importance of nineteenth-century precedents in paving the way for the changes he discusses. Inland recruiting, specialized training, recognition of individual worth, and the tempering of justice with mercy were all the subject of proposals or experiments in the previous century. The concept of continuous service dates from 1855. Because of it, there were old seamen in the late nineteenth-century who proudly identified themselves with the Navy. The Navy rarely rushed into any experiment.

It would have been interesting if the author had gone a bit further on some points. In *The Search for Order* (1967), Robert H. Wiebe saw the establishment of the Great Lakes Training Station as an effort by wealthy Chicagoans, scared by the Haymarket riot, to bring federal power to that city. Harrod does not mention this argument, but his treatment of the topic does not tend to support Wiebe. Likewise it would be useful if more had been said about the role of religion. Almost certainly the increase in Catholic chaplains was both a belated recognition of need as well as a result of the gradual awareness by Navy leaders of the Church's role as a conservative, stabilizing influence in society. Yet these criticisms do not detract from the fact that this book is a welcome addition to naval history and to aspects of a broadly conceived social history of the United States.

HAROLD D. LANGLEY
Smithsonian Institution

EDWARD B. PARSONS. *Wilsonian Diplomacy: Allied-American Rivalries in War and Peace*. St. Louis: Forum Press. 1978. Pp. ix, 213. \$9.95.

Many Americans once believed that Woodrow Wilson was an idealistic innocent, a Christian

thrown into the lions' den of international affairs to suffer inevitable defeat at the hands of wily foreign leaders at Versailles. As scholars dug into the sources, it became apparent that the American president was not so naive and indeed made conscious efforts on behalf of American interests. This should not be surprising, given Wilson's intelligence and position; however, some may find Edward B. Parsons's concept of Wilson shocking. In *Wilsonian Diplomacy* Parsons depicts a man who, driven by economic determinism and suspicion of the British, attempted suppression of American aid to the Allies during World War I in order to ensure a postwar pre-eminence for his nation.

The author fills his monograph with figures and quotations drawn from extensive research in American and British diplomatic and naval sources. Others who have used some of the same sources might take issue with Parsons's selection of quotations and his interpretation. There is no question, nevertheless, that American leaders considered economic matters and that some harbored a good deal of anti-British sentiment.

While Parsons deals with a wide variety of diplomatic and naval issues through the war and the peace conference, the heart of his thesis is his charge that Wilson deliberately throttled the American war effort. In this critical area the author's failure to use American military records and scholarly monographs—he seems to have relied largely on a 1931 journalistic biography of the Secretary of War for military matters—severely hurts, if not destroys, his case. In the first place, whether Americans would fight as replacements in Allied ranks or as an independent army was a much more complex issue than Parsons suggests. Then, most significantly, the logistical difficulties of American military aid simply do not sustain the thesis that trained men and materiel were available in quantity before the spring of 1918. It takes months to turn industry to the production of war materiel as well as to raise, train, and equip a large army. Finally, there is the point that serious British requests for American troops came only late in 1917, after the failure of their own offensives, the Italian defeat, and the Russian collapse.

When one takes these factors into consideration, the reasons for American military aid arriving so late (albeit still in time to tip the balance) would seem to be other than the manipulations of a machiavellian Woodrow Wilson.

EDWARD M. COFFMAN
University of Wisconsin,
Madison

JOHN M. MULDER. *Woodrow Wilson: The Years of Preparation*. Princeton: Princeton University Press. 1978. Pp. xv, 304. \$16.50.

John M. Mulder's is the most thorough and perceptive account yet written of Woodrow Wilson's career prior to his active entry into politics. Except for his treatment of Wilson's father, the significance of his study lies not so much in his having uncovered new data as in his skillful use of the materials in the marvelously comprehensive *Papers of Woodrow Wilson*, edited by Arthur S. Link and his associates at Princeton, to trace Wilson's intellectual development during those crucial formative years.

The central theme of this work is the all-pervasive influence of Wilson's religious faith upon his thinking and behavior. The point is not new. Ray Stannard Baker stressed the importance of Wilson's religion. More recently, Link has, in a series of articles, explored the influence of Wilson's Calvinist heritage. Mulder's contribution is in pinpointing more specifically that heritage as "the Presbyterian covenant religious tradition" (p. xiii). That tradition did not supply Wilson with a ready-made set of theological ideas—for he was remarkably uninterested in metaphysical problems—but rather with "a series of values, assumptions, and attitudes, a way of perceiving the world and understanding his place within it" (p. xiii).

Mulder traces how Wilson derived from covenant theology persisting themes in his own thinking: his passion for order, structure, and organic wholeness; his emphasis on the freedom and responsibility of the individual; his belief that man's duty was to battle in this world for God's cause against the forces of evil; his ambition to integrate morality into the affairs of government; his conception of the role of the charismatic leader in changing the course of history; and what Mulder aptly calls "a profound conservatism that tested ideas and institutions of the past against the needs and demands of the present" (p. 49).

At the same time, he shows how the covenant heritage was sufficiently ambiguous, vague, and flexible to accommodate differing emphases in response to differing circumstances. Mulder points out that in the covenant tradition and within Wilson "there was a tension between power and order on the one hand and freedom and shared authority on the other. . . . When disorder threatened him personally or the organic nature of society, he responded by stressing the need for integration and power. When disorder subsided and power appeared to become coercive, he emphasized freedom and individualism" (p. 170).

Mulder does a brilliant job of delineating how Wilson's religious heritage interacted with his life experiences—his successes and disappointments, his physical ailments, and the impact upon him of such external influences as the tumultuous events of the 1890s—to shape his thinking over time. Per-

haps most interesting, and illuminating, for students of Wilson's later political career is Mulder's analysis of how his 1906 stroke and his defeat in the battles over the quadrangle plan and the graduate college pushed him from his former conservatism toward a more progressive stance.

While he is sympathetic to Wilson, Mulder does not ignore his shortcomings: his penchant for reducing all issues to moral questions, his tendency to make his own conscience the final test of right and wrong, and his resulting intolerance toward those who disagreed with him. The book has its weaknesses. Mulder's largely chronological approach coupled with his overly extensive quotations from Wilson's writings and speeches tends to blur the focus of his analysis. He is prone to exaggerate the significance of shifts in emphasis that may have reflected more Wilson's adaptation to this audience than any substantive change of mind. And his concentration on the inner dynamics of Wilson's thought results in an underestimation of the influence of the surrounding intellectual milieu.

These caveats notwithstanding, Mulder deserves high praise for making not only a major contribution to our knowledge about Wilson, but also for enhancing our understanding of the role of religion in American life.

JOHN BRAEMAN
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RONALD H. BAYOR. *Neighbors in Conflict: The Irish, Germans, Jews, and Italians of New York City, 1929-1941*. (Johns Hopkins University Studies in Historical and Political Science, ninety-sixth series, number 1.) Baltimore: Johns Hopkins University Press. 1978. Pp. xiv, 232. \$14.00.

In this fact-filled book, Ronald H. Bayor has made a substantial contribution to our understanding of one of the endemic problems of New York City and, indeed, of all major cities: the tensions arising from the interaction of ethnic and racial groups in the urban setting. From the title, *Neighbors in Conflict*, one might expect the emphasis to be upon friction at the neighborhood level, but relatively few examples of this are given. The bulk and strength of the book are Bayor's elaboration of the role of organizations, more or less city-wide in nature, such as the Christian Front, the German-American Bund, the American Jewish Committee, the Catholic Church, the ethnic-oriented press, and local political parties—to mention only a few—in aggravating tension or helping to mitigate it.

The depression at home, the rise of Nazism and

fascism abroad, and the debate over American involvement in World War II were the environmental and emotionalizing factors that conditioned the interaction of the major ethnic groups in New York City between 1929 and 1941. Irish-Jewish tension reached the point of violence in these years as the Irish, especial victims of the depression, resented the economic and political advancement of New York's Jewish residents and saw a threat to Catholicism in communism, with which some Jews were identified—an attitude that was encouraged by Father Coughlin and others and that the Church, as an institution, was slow to criticize. Germans and Italians in New York were less openly anti-Semitic lest this hurt their nationality image or because they were more vulnerable than the Irish to potential economic reprisals from members of the Jewish community.

Bayor relates these developments in detail, as he does the role of ethnic issues in the play of city politics. His coverage of the mayoral campaigns of 1933, 1937, and 1941 shows how Fiorello La Guardia manipulated ethnic issues to maintain crucial Jewish support, even at the risk, in 1941, of weakening his hold on the Italian vote. Anti-Semitism, Nazism, and communism were explosive issues in all of these municipal campaigns, and their exploitation by the candidates intensified group antagonism and conflict.

In explaining both the rise and resolution of group conflict, Bayor stresses the role of what he calls the "vital interests" (real or imagined) of an ethnic group. The threat to what they considered their "vital interests" prompted the Irish to engage in overt and violent anti-Semitism. New York Germans ultimately saw it to their advantage to take an anti-Nazi stand lest silence inspire a revival of the anti-German excesses of World War I. Motivations other than material interests might well have been suggested.

Bayor's coverage of the activities of scores of ethnic and religious societies, as well as pertinent newspapers and magazines, is impressive. In conceptualizing conflict, he might have made more of the role of the urban community, as such, in spawning organizations that make group antagonisms articulate and dangerous—such as the Irish-dominated Christian Mobilizers—and in fostering societies—like the Bureau of Goodwill Between Italians and Jews, set up by the American Sons of Italy—that on occasion help to ease group tension.

BAYRD STILL
New York University

ROBERT E. HUMPHREY. *Children of Fantasy: The First Rebels of Greenwich Village*. New York: John Wiley and Sons. 1978. Pp. ix, 267. \$14.95.

In writing the intellectual history of a movement or a school of thought, the historian is obligated to show the connections among the holders of the ideas. Which aspects of the more general philosophy did they all share? In what ways did they differ? Robert E. Humphrey's *Children of Fantasy* notes the connections between the five men chosen for the study: Floyd Dell, Max Eastman, George Cram Cook, Hutchins Hapgood, and John Reed but does not focus upon the shared philosophy. Rather, this study is a loosely structured series of biographical portraits of each man with an introductory chapter that summarizes already published information on the Village. The conclusion offers a superficial critique of the men as self-indulgent adolescents who never outgrew their fantasies of personal power and the creation of a utopian society.

The connective threads of the book are slim; Humphrey suggests that all had lonely childhoods and adolescences, and during those miserable times they all weaved fantasies of adventure and power which they hoped to translate into reality as adults. Hardly a novel problem. The simplistic reductionism of the thesis is matched by the formulaic writing of each of the five central chapters describing the lives of each of the protagonists. Each chapter opens with a description of the hero's problematic childhood followed by his trials growing up and his attainment of manhood in Greenwich Village. Four of the five attended Harvard, a fact not discussed, though given the tenuous connections of the book, one upon which much could be made.

The conflict between personal fulfillment and social activism is a recurrent one for all intellectuals who hope to be activists as well. But Humphrey sets up a straw man. Except for John Reed, the men discussed in this book viewed themselves primarily as writers and social thinkers, not as activists. They were writers who enjoyed living in Greenwich Village according to their own lights and calling society's attention to social ills. They were often naive and romantic, adjectives they used on each other. Humphrey criticizes their self-indulgences and their inexperience in politics, though he fails to note that their enthusiasm for the strikers in Paterson, Ludlow, and New York City and for the feminist movement placed them among the brave but few supporters of these controversial movements. Further, Reed's and Eastman's miscalculations on Lenin and the Bolshevik Revolution were shared by many on the liberal left in the early twenties. It took the brave Emma Goldman to denounce Lenin very early. But then Humphrey treats the vibrant feminists only peripherally. He glances at the women as they affect the men, a peculiarly old-fashioned pose.

John Diggins's study of Max Eastman in *Up From Communism* (1975) was not cited in the bibliography, a curious omission. *Children of Fantasy* adds no light to the subject of the Greenwich Village rebels. Since no significant connections were made in this book, the author's final attack severs nothing but his own questionable thesis.

JUNE SOCHEN

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MARTIN WEIL. *A Pretty Good Club: The Founding Fathers of the U.S. Foreign Service*. New York: W. W. Norton. 1978. Pp. 313. \$12.95.

In the first half of the twentieth century, America's corps of professional diplomats consisted of a small, homogeneous group of Christian gentlemen who had known one another for years, often called themselves the Family, and sneered down long noses at any non-diplomat who sought to understand or conduct foreign affairs. Part of these diplomats' condescension toward outsiders derived from their desire to be the only men allowed to conduct foreign policy and part from their resentment that the rest of the country had them pegged correctly as a pack of lazy, ignorant snobs. Now, with Martin Weil's book, *A Pretty Good Club*, we learn just how lazy, ignorant, and snobbish these fellows really were.

Weil traces the careers of central figures of the diplomatic branch of the foreign service from the end of the First World War until the beginning of the Cold War, with the bulk of his study devoted to the diplomats' tribulations in the administration of Franklin D. Roosevelt. Weil has organized his book as a series of portraits of the more obnoxious and anti-Semitic of the diplomats—Arthur Bliss Lane, Joseph C. Grew, Jay Pierrepont Moffat, William Phillips, and others. It makes for delightful reading to follow Lane's bigoted career in Poland or to learn of the bureaucratic backbiting in Washington during World War II. What emerges from these sketches is an impression of an embattled elite, a club, suspicious of outsiders and motivated by desires for self-promotion and by deep anti-Semitism.

The anecdotal approach which Weil uses carries the reader along a merry stream of ironic, even contemptuous writing, but sometimes it gets in the way of understanding. First there is the problem of figuring out the internal dynamics of the state department. Although at the end of his book Weil does include five organizational charts of the state department, he nowhere discusses in any detail the efforts at organizational reform that occurred from the Rogers Act of 1924 until the Reorganization Act of 1946. He makes only the briefest of mention

of the deep hostility between consuls and diplomats in the twenties. While he offers an intriguing vignette of the back stabbing between the diplomats and the experts of the OSS during the Second World War, he does not generalize about the fear foreign service officers have always had of experts who got their training outside the Department of State. Finally—surprising for an author who clearly thinks that these diplomats were fools—Weil gives them too much credit for creating the atmosphere that led to the Cold War. As Weil rightly points out, few astute political observers ever took these diplomatic clubmen seriously; surely they could not have been the major architects of the postwar break with the Soviet Union.

Weil has based his conclusions upon the diplomats' private correspondence and upon interviews with over a dozen of them. He makes little use of their actual dispatches or the extensive public documents about foreign service reorganization. His interviews, however, are splendid. Sometimes he quoted where he should have paraphrased, but in one case especially, that of the OSS experts under H. Stuart Hughes, he told an important story with wit and grace.

ROBERT D. SCHULZINGER
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HAMILTON CRAVENS. *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900-1941*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xvi, 351. \$17.50.

The appearance of *The Triumph of Evolution* marks the entry of a promising scholar into the field of the history of American science. Hamilton Cravens has taken up again themes pioneered by Stow Persons, ed., *Evolutionary Thought in America* (1950).

Cravens approaches the heredity-environment controversy, colloquially called the nature-nurture controversy, first by examining the history of the disciplines of biology, psychology, anthropology, and sociology in the period from approximately 1890 to 1920 and then by discussing the themes of race, instinct, and mental testing to bring his story chronologically to 1940. The major setting for the controversy was the American university, and many of the battles were fought under the rules of academic politics. Cravens neatly describes these institutional conflicts and goes on to weigh the thought of the major actors, with an eye for the larger social meaning of ostensibly scientific data.

In the wake of the rediscovery of Mendel in 1900, an emphasis on heredity and support for eugenics blossomed among biologists, with Charles B. Davenport the prime example. Later the experimental-

ists, led by T. H. Morgan, moved biology away from involvement with conservative reform and immigration restriction by sheer dint of laboratory results.

In psychology an older generation of founders—William James and G. Stanley Hall—gave way to younger men who divorced experimental science from philosophy. Robert M. Yerkes and the young psychologists who administered the testing program of the army in World War I shared many of the hereditarian tendencies of the biologists whose methods they emulated, but the logic of their developing behaviorism led them ultimately to dump the concept of instinct and to try to eliminate the cultural biases embedded in their mental tests.

The dominant figure on the side of environment, or more precisely on the side of culture, was Franz Boas. Cravens draws together the work of George Stocking and others to document in detail the divorce between biological and social theory that opened the way for the concept, as R. H. Lowie put it, that an anthropologist "will account for a given cultural fact by demonstrating some other cultural fact" (p. 89) and not by looking to biological evolution for an explanation.

Although sociology emerged from the nineteenth century with many ties to reform movements and to undergraduate teaching in the universities, it ultimately defined itself as a field as far from biology as was anthropology. According to William F. Ogburn, the sociologists "consider social evolution as not including, at least in the past several thousand years, any biological evolution" (p. 122).

On two counts Cravens might be criticized. He rightly sees that the social and economic background of his scientists was important in directing their ideas, even their "scientific" findings, on the subjects of heredity and environment and their social applications in the form of eugenics, mental testing, and immigration restriction. Nowhere, however, does he analyze that social and economic background, evoking rather the acronym WASP and adding the modifier "middle-class." This axiomatic causal explanation, itself a confused mixture of nature and nurture, is used as an all-purpose adjective. In one place his scientists are "native-born Americans of Anglo-Saxon ancestry" (p. 54); in another they belong to "middle-class Anglo-Saxon culture" (p. 74).

The second limitation, which stems in part from the first, is a positivist belief that the data gathered in the laboratory led the scientists out of their culture-bound beliefs to a new evolutionary synthesis—the "triumph" referred to in the title—that resolved the heredity-environment controversy completely. Cravens gives only the slightest hint that he foresaw the revival of a hereditarian orthodoxy in the Shockley-Jensen school of

mental testing and in E. O. Wilson's sociobiology. Far from being resolved, the heredity-environment controversy is still with us, and the only discernible progress is that books such as Cravens's give us an awareness of the historical dimensions of our predicament.

A. HUNTER DUPREE
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CHARLES A. LINDBERGH. *Autobiography of Values*. Edited by WILLIAM JOVANOVIICH and JUDITH A. SCHIFF. New York: Harcourt Brace Jovanovich. 1978. Pp. xxi, 423. \$12.95.

Shortly before his death in 1974, Charles Lindbergh, typically well organized, consulted friend and publisher William Jovanovich about completing his memoirs. The result is this posthumous *Autobiography of Values* which reflects well the life and views of the pioneer aviator. It discusses his youth in Minnesota, civil and military aviation, scientific research, family, the press, World War II, and his later interests in conservation, primitive peoples, and mysticism. Among those whose lives or writings influenced Lindbergh were Alexis Carrel, medical researcher; Lao Tse, early Chinese mystic philosopher; and his father, reform-minded Minnesota congressman and scholar. Evidence of Lindbergh's superb technical knowledge and substantial leadership in aviation development is clear. He participated in many crucial decisions from the selection of aircraft and routes in early commercial flight to the introduction of jets and the structuring of the Strategic Air Command.

But the heart of Lindbergh's reflective study emphasizes his growing concern that modern technology endangers the environment—a conflict he describes as “civilization” versus the “primitive,” or, put another way, “intellect” against “instinct.” Thus Lindbergh, aviator and technician, gave rise to Lindbergh, conservationist and defender of wildlife. His struggle with changing values is also seen by his support of U.S. retaliatory power during the Cold War and his worry that aviation had made all peoples vulnerable to atomic annihilation. As a scientist, his values included a continuing confidence that “nature is prolific, competitive, and selective” (p. 195). Increasingly in later years he turned from science to mysticism and the primitive in seeking answers to the meaning of life and death and the universe. Nevertheless, he still dreamed of space travel and understanding beyond human rationality.

Certain characteristics of Lindbergh and his values emerge—an incessantly inquiring mind, a total sincerity, and a meticulous attention to detail and accuracy. These qualities, along with a stub-

born independence, a sense of courage, and a quiet personal nature, were also typical of his father.

Lindbergh's prose style is marked with beauty and power. It is at its best when assessing firsthand experiences, whether it be landing in a Southwestern dust storm with Anne, picking up spear-carrying Masai tribesmen in his VW, or piloting a fighter in the South Pacific. Moreover, the flashback technique used in *The Spirit of St. Louis* (1953) is quite effective here.

Editors Jovanovich and Judith Schiff, Yale archivist, have done a solid job in selecting from almost 3000 pages of manuscript. There are a few instances of repetition; it might also have been helpful to indicate a specific time of authorship for sections of a book written over roughly forty years. Good photos, maps, and a reasonably complete bibliography of Lindbergh's publications enhance the volume. *Autobiography of Values* is an essential work in understanding Lindbergh and his times.

BRUCE L. LARSON
Mankato State University

B. Y. MIKHAILOV *et al.* *Recent History of the Labor Movement in the United States, 1918-1939*. Moscow: Progress Publishers; distributed by Imported Publications, Chicago. 1977. Pp. 533. \$8.00.

The authors of *Recent History of the Labor Movement in the United States*, “a group of Soviet historians and economists,” have produced a book bereft of subtlety and bristling with pronouncements. Throughout the 514 densely packed pages, virtuous workers confront capitalists, while a bourgeois government vacillates between repression and co-optation. In the many struggles of the interwar period, Communists invariably lead the way, achieving the proper balance between revolutionary enthusiasm and sober regard for practical realities. Non-Communists are frequently duplicitous, always misguided. Particular disdain is reserved for the corrupt, ignorant, and complacent misleaders of the class-collaborationist American Federation of Labor.

Although the Communist Party is ubiquitous in this volume, its internal structure and dynamics are not analyzed. Bourgeois historians of the party, such as David Shannon, Theodore Draper, Irving Howe, and Lewis Coser, are summarily dismissed. The name Joseph Stalin appears nowhere in this book. Serious scholars who consult *Recent History of the Labor Movement* will want to do so in tandem with Bert Cochran's *Labor and Communism* (1977).

Despite its party-line character, the book has virtues. It will do American historians no harm to read an account of the 1920s and 1930s in which

workers play the central role. The authors pay serious attention to the place of Afro-Americans in the labor force and the social movements of the period. Their extensive bibliography and a combative historiographical essay indicate that they have done their homework (although there are no references to American works published after 1967), but large sections of the text proper appear to rely on Communist Party materials and the writings of sectarian authorities.

At its best, as for example in the treatment of labor unrest after World War I and again in the early New Deal period, *Recent History* is lucid and informative. At its most dismal, it is a blatant apology. Treatment of foreign policy-labor issues of the 1930s, for example, evades hard questions, distorts the international context, and ignores the internal circumstances prevailing in the Soviet Union. The volume's tone is self-righteous, omniscient, and schematic. Phrases such as "the workers" and "progressive" are regarded as self-defining. No New Leftist nuances cloud these pages. No concessions to pluralism are made.

Readers will find coverage of most important trends and episodes. Space allocation, however, is questionable: Communist Party activities receive extensive treatment, while even central events such as the Flint sit-down and the Republic Steel massacre are dealt with summarily. The prose is too often seriously misleading: Victor Berger opposed American participation in World War I, contrary to the authors' implication otherwise (p. 47); Steel Workers Organizing Committee victories in 1936-37 were much less sweeping and much more difficult to extend than the authors suggest (p. 381); the nature and impact of various philosophical systems on the mind of the American worker cannot be assessed in the terms employed by the authors (p. 147).

Recent History serves as a Soviet version of American labor history in the interwar period as seen from the mid-1960s. It deserves bookshelf space for its summaries of economic trends and its many quotations from Communist Party declarations. It is worth reading in the way that a book resting on absolute conviction often challenges and unsettles. It is not, however, a particularly edifying example of the historian's craft, at least if the values associated with that endeavor include sensitivity to alternatives, awareness of diversity, and respect for the presumed integrity of historical actors.

ROBERT H. ZIEGER
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HARVEY SCHWARTZ. *The March Inland: Origins of the ILWU Warehouse Division, 1934-1938*. (Industrial Relations Monograph Series, Number 19.) Los

Angeles: Institute of Industrial Relations. 1978. Pp. xiii, 262. Paper \$7.50.

In May 1934, the International Longshoremen's Association (ILA-AFL) began a maritime strike that closed West Coast ports for three months. In San Francisco, ILA Local 38-79, under the leadership of Harry Bridges, had difficulty making the strike effective because of the lack of cooperation from organized Teamsters and unorganized workers in the numerous dockside and inland warehouses. Within a short time, Local 38-79 began "the march inland" by attempting to organize Bay Area warehousemen. Terrible working conditions made warehousemen receptive to the organizing efforts of the ILA "militants." Bitter employer opposition, including both single industry and multi-industry anti-union associations, was gradually overcome. The "militants" eventually organized the warehousemen in the hardware (a "major testing ground"), wholesale drug, electrical supply, and numerous other industries. Final victory came early in 1937 after a sixty-seven-day strike.

Even as warehouses near the waterfront were being organized, Bridges and the militants formulated plans to carry the San Francisco organizing drive to every port on the West Coast. They envisioned the creation of a giant industrial union of marine, distribution, transportation, and agricultural workers, including the Teamsters. Toward this end, in 1935 they formed the Maritime Federation of the Pacific. But such plans disturbed the conservative ILA leadership on the East Coast and the West Coast Teamsters, which were both exponents of AFL craft autonomy. Conflict within the ILA was settled in 1937 when the West Coast maritime unions withdrew from the AFL and affiliated with the International Longshoremen's and Warehousemen's Union (ILWU-CIO). But Teamster opposition was more formidable, particularly that of Seattle-based Dave Beck. Under his leadership, the West Coast Teamsters not only halted the ILWU effort to organize inland packing houses, but also initiated an eventually unsuccessful drive to capture San Francisco warehousemen already organized under the ILWU banner. Thus, by 1938, it was not powerful employers' associations that brought the march inland to a halt, but the rival Teamsters. Stalemated, the ILWU march was disbanded by the end of that year.

A considerable amount of research went into the preparation of this account, which began as the author's dissertation. The ILWU archives were used extensively, but of particular importance were interviews conducted by the author with former ILA-ILWU militants. Harvey Schwartz balances these admirably with those provided by

employers drawn from oral histories at the University of California-Berkeley. Unfortunately, after the first thirty pages, the earmarks of a dissertation are too obvious. The absence of an index is a serious fault and much of the text is burdened with lengthy, undigested quotations. Numerous footnote citations (eighty-three pages in length) could have been somewhat shortened with the use of the simple word "*ibid.*" Nevertheless, *The March Inland* does provide a needed scholarly addition to the expanding list of published material on West Coast unionism.

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HARRY HAYWOOD. *Black Bolshevik: Autobiography of an Afro-American Communist*. Chicago: Liberator Press. 1978. Pp. x, 700. \$15.00.

Born in 1898 in Nebraska, Harry Haywood grew up there in an environment relatively free of prejudice. Although he had been a good student, he quit school at fifteen and found work on railroad dining cars. In early 1917 he enlisted in a black National Guard regiment in Chicago, was called to active duty later in the year, was at the front in France, became sick, and learned something of French language and life during an extended stay in a Paris hospital with delightfully lax discipline. Released from the army, he resumed his life in Chicago as a dining car waiter. Shocked by the Chicago race riot of 1919 and dismayed by the contrast between his Chicago and Paris experiences, he joined a leftist study group. In 1922 he told his older brother Otto, who had been a member of the Communist Party for about a year, that he wished to join. He accepted Otto's advice to join the African Blood Brotherhood, but the next year he joined the Young Communist League and in 1925 joined the party itself. Within months he was selected to study at a Moscow school for foreign Communists, KUTVA, often known to American Communists as the Far Eastern University. The following year he was transferred to the Lenin School, and while there worked for the Comintern. Haywood was in Russia from April 1926 until October 1930. After his return he advanced rapidly within the party's structure but did not reach its highest levels, although he became the head of the National Negro Department in 1931 and a member of the Politburo in 1934. Within a few years, however, his fortunes within the party declined, primarily because of his zeal for a special Communist kind of black nationalism. This idea, originally Stalin's, can best be summarized briefly by the slogan adopted in 1928 by the Sixth World Congress of the Comintern, "the right of self-determi-

nation of the Negroes of the Black Belt." Haywood pressed hard to get the American Communist Party to come out loud and clear for self-determination, but its most important white leaders as well as many of its blacks remained ambiguous in their support even when self-determination was part of the party line. Finally, in 1958, after the party crumbled in the wake of Khrushchev's "secret speech" at the Twentieth Congress of the CPSU and the intervention in Hungary, the party dropped self-determination and expelled Haywood as an "ultra-leftist." In this book Haywood does not declare himself a Maoist, but he quotes the Chinese leader extensively.

What use can historians make of this book? For one thing, when Haywood relates his personal life history he tells an interesting story; on the other hand, when he uses "Communes," as he invariably does when dealing with political matters, the prose is almost impenetrable. His account of life within the party tells us nothing important we had not already known, but he provides new details that have the ring of truth. Obviously, his reconstructions of decades-old conversations, written as if they were verbatim, are spurious, and as with any autobiography we are left wondering if there are important elements of his life omitted.

DAVID A. SHANNON
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GENE M. GRESSLEY, editor. *Voltaire and the Cowboy: The Letters of Thurman Arnold*. Boulder: Colorado Associated University Press. 1977. Pp. xiv, 552. \$15.00.

In his path-breaking study of American reform movements, *The Age of Reform* (1955), Richard Hofstadter chose Thurman Arnold as his quintessential New Dealer, a person whose social philosophy and political deeds stood in sharp contrast to those of the Populists and Progressives. Whereas these earlier reformers, drawn from the nation's countryside and small towns, were nostalgic, intensely moralistic, and frequently doctrinaire in their approach to economic and social issues, the men and women who formed the core of Roosevelt's New Deal exemplified the modern spirit of scientific inquiry, bureaucratic rationality, and social cynicism. For Hofstadter, the New Dealers were result-oriented, hard-boiled managers, frequently social outsiders, drawn disproportionately from the professions of law and engineering, who set about solving the economic crisis of the Great Depression without the humorless fanaticism or paranoid delusions of their reform-minded predecessors. Who better symbolizes his hypothesis than Arnold, the wise-cracking, cigar-smoking law professor from

Yale, via Laramie, Wyoming, leader of the decade's anti-trust crusade and author of two irreverent books, *The Symbols of Government* (1935) and *The Folklore of Capitalism* (1937), lampooned the conventional wisdom of economic and political life?

This sample of Arnold's voluminous correspondence, superbly annotated with an extended biographical essay by Gene M. Gressley, both confirms and modifies Hofstadter's portrait of a mischievous and talented reformer. The letters cover the years from 1910, when Arnold was a student at Princeton, until shortly before his death in 1970. By far the best entries, liberally sprinkled with their author's hyperbolic observations on economic, political, and legal affairs, focus upon the late 1930s when Arnold served the New Deal as an assistant attorney general in charge of anti-trust enforcement and the post-World War II years when the senior partner in Washington's influential law firm of Arnold, Fortas, and Porter helped to spread the benefits of Coca-Cola to the Third World and courageously defended many government employees against the witch-hunting machinery of the White House, loyalty boards, FBI, and Congress.

As a young lawyer-politician in Laramie following World War I, Arnold adopted a reform program that echoed the practices and cultural values of many prewar municipal reformers, including a "vice war" against the purveyors of liquor, prostitutes, and gambling. The son of a prosperous attorney and rancher, Arnold still shared with many New Dealers the social mentality of the outsider who hoped to become an insider. Never as poor as other young men of his generation from the provinces (William O. Douglas, Robert Jackson, Lyndon Johnson, and Marriner Eccles, for example), Arnold nonetheless spent much of his life waging a guerrilla war against the financial, political, and intellectual establishments of the East.

His anti-trust views were likewise derived from the progressive era and paralleled those held by Woodrow Wilson. For Arnold, vigorous enforcement of the Sherman Act did not demand a blind assault against big business or a morbid concern for the welfare of consumers. In his opinion the anti-trust laws should be used to strip away special privileges and entrenched business practices that retarded economic innovations and thwarted the ambitions of more aggressive, growth-oriented entrepreneurs. Along with Jesse Jones, Eccles, and other New Deal spokesmen for cowboy capitalism, Arnold had nothing but contempt for the rentier class of businessmen and the older bastions of corporate power. His heroes were Henry Ford, Western steel king Henry Kaiser, youthful railroad tycoon Robert Young, and Robert Woodruff of Coca-Cola. Beginning in his college days at

Princeton, however, Arnold also struggled ceaselessly to acquire the education, tastes, and income of the upper class by building up his personal reputation, extending his legal practice, and making the right social connections in New Haven, Cambridge, New York, and Washington.

Arnold's old-fashioned individualism (sometimes bordering upon paranoia in the case of certain labor unions and their leaders) served him well after 1945. At a time when less fearless New Dealers capitulated to the excesses of McCarthyism, he defended Dorothy Bailey and Owen Lattimore, two of the more tragic victims of the loyalty-security purges under Truman and Eisenhower. His lawyer's mind, finally, could not comprehend the street demonstrations and campus unrest generated by the Vietnam War during the late 1960s. Although he admired the courage of Senator Ernest Gruening, an outspoken critic of the war, he remained loyal to President Johnson's policies and bitterly condemned the young people who took direct action against those policies. In view of all that he had written earlier about the importance of symbols, myths, and the irrational in public life, these final letters are both surprising and depressing. Arnold failed to understand that the legal order which he so admired had ignored the gravest moral and constitutional problems associated with the war and that many young men and women, through no fault of their own, had stopped believing in the dominant symbols and myths of American foreign policy—much as an earlier generation had rejected the shibboleths of capitalism during the Great Depression.

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San Diego

JOHN MODELL. *The Economics and Politics of Racial Accommodation: The Japanese of Los Angeles, 1900-1942*. Urbana: University of Illinois Press. 1977. Pp. xii, 201. \$10.00.

Seldom does an author candidly call attention to the most serious shortcomings of his book in his preface. Yet this is what John Modell does in his study of two generations of Japanese-Americans in Los Angeles between 1900 and 1942. He apologetically warns the reader that, while his analysis is firmly based on information collected while he worked for the Japanese-American Research Project at UCLA, he neither reads Japanese nor can he claim any deep insight into the Japanese-American mentality or the lives of individuals. Instead his "central concern was accommodation as a public position" (p. xi). The result is a book that carefully and competently explains occupational

patterns, economic opportunities, intergenerational conflicts, organizational structures, and racial friction but that gives little sense of the personal lives that made up this history. It is unfortunate he chose such a cold, impersonal approach, for the study of the racial accommodation of Japanese-Americans prior to World War II needs to be told both to a public that still associates Japanese with Pearl Harbor and to professional historians who often ignore the Asian immigrant. That story, Modell admits, "would require a command of Japanese, and will take a scholar who is willing to take on and make sense of the undoubtedly disparate recorded viewpoints of first-generation participants in the evolution of a community" (p. xi).

Within the limits that the author set for himself, however, he has done a superior job. The choice of Los Angeles is excellent because the Japanese-American population there was large, but racial discrimination was less harsh than elsewhere. The *Issei*, accepting a middle position between the Caucasians at the top and the Negroes and Chicanos below them, exploited economic opportunities in agriculture, fishing, gardening, and fresh produce marketing to create a stable, though modest, "ethnic economy." By taking advantage of those occupations and businesses ignored or rejected by white Americans, they expected, through hard work and personal sacrifice, to eventually achieve their part of the American Dream. As long as racial discrimination remained mild, the first generation immigrants were able to progress slowly through diligence and support of one another in the community.

The second generation of Japanese-Americans, however, were more aware of the limits of their parents' accommodation. Although the *Nisei* worked hard, acquired a better-than-average education, and began to take over the occupational structure of their parents, their expectations of economic and social mobility were largely unfulfilled. Like the children of so many European immigrants, they found their parents' experiences irrelevant to their needs in becoming more Americanized. The Japanese American Citizens League was the organization that expressed the attitudes of the *Nisei* who refused to accept the racial accommodation of the *Issei*.

If the depression weakened the ethnic economy of the Japanese-Americans in Los Angeles County, World War II was the crushing blow. Modell points out that the Japanese-Americans, especially the *Nisei*, tried desperately to overcome their public image of possessing dual loyalties. But, like the German-Americans of World War I, they had little success.

Ending a book about Japanese-Americans in

1942 just as the relocation was about to begin was a wise decision. By avoiding the emotional uprooting that occurred during the war, Modell is able to keep this readers' attention on his main theme, that of "the accommodation of one minority to racist America . . ." (p. 190). Less wise was the decision to omit photographs and bibliography. A few pictures would add much-needed life to the book, and a bibliography seems necessary on a topic not widely known.

DEAN R. ESSLINGER
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LISLE A. ROSE. *The Long Shadow: Reflections on the Second World War Era*. (Contributions in American History, number 70.) Westport, Conn.: Greenwood Press. 1978. Pp. 224. \$16.95.

This book is a series of eleven original essays about modern times, more or less connected by the theme of the impact of World War II on the world scene, especially America. Not surprisingly, Lisle A. Rose concludes that the war was the central event of the century, casting a shadow that stretches out to today and into the future. If he offers no original scholarship, this is a thoughtful work containing a few useful insights and some thoughtful observations.

Rose begins with "The Age of Hitler," asserting that popular thought in the West today has been shaped by five secular messiahs, Jefferson, Adam Smith, Marx, Darwin, and Hitler. Rose contends that Hitler legitimized three roads to power: vengeance, violence, and the idealization of the totalitarian warfare state. Taken together, these developments brought on a revolution in international life and extended the impact of Hitler into the private lives of everyone in the West. An inevitable comparison with Nixon is drawn.

The remaining essays cover a broad range with little depth. An essay on "Grand Strategy" argues forcefully that the U.S. Navy should have speeded up its campaign against Japan, using a "single thrust" rather than a "broad front" approach to the home islands. In "The Two Cold Wars," Rose blasts the New Left revisionists, not for their careless handling of the sources, but for their myopia with regard to the real situation that existed in the postwar years. He also contends that there were two cold wars, one from 1945 to 1949, the other from June 1950 to the present. "The first cold war was limited, comparatively clean, and defensive. The second cold war was global, dirty, and frequently aggressive" (p. 146). Other essays deal with the outbreak of World War II, the "Communist Myth in America," and America's role in postwar Asia.

Rose is not above hyperbole. In an essay on "The Last Warrior," (John F. Kennedy), Rose writes that Kennedy "was deeply hated at his death by all who, in one way or another, feared and despised the simple adventure of living well and freely . . ." (p. 186). Such nonsense, it is only fair to say, does not characterize the book as a whole.

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RICHARD C. LUKAS. *The Strange Allies: The United States and Poland, 1941-1945*. Knoxville: University of Tennessee Press. 1978. Pp. viii, 230. \$12.50.

The vagaries of American policy toward Eastern Europe in World War II have been chronicled and analyzed by a multitude of participants and scholars. Richard C. Lukas thus goes over well-tilled ground in relating the story of Washington's responses to the Polish problem, the single most important part of the East European puzzle confronting the Allies.

Poland, as one of the minor Allies, could expect more consideration than some of its East European neighbors that were at least nominally at war with the United States. Polish armies fought bravely on several fronts, a government-in-exile in London functioned as the official voice of occupied Poland and as the guide of the underground Home Army, and a large Polish-American community served as a vocal defender of Polish interests. Despite these tangible assets and the promises of the Atlantic Charter, Poland's territorial integrity and chance for representative government were sacrificed on the altar of Allied solidarity. These are the themes of Professor Lukas's admirably concise and objective study.

Arguably, Poland's fate was predetermined by its location in the Soviet sphere of military operations and by Stalin's determination to secure territorial concessions and a "friendly" postwar regime. The historical animosity between the two countries had been exacerbated by the Polish military campaign against Russia of 1920-1921, the Treaty of Riga, and the anti-Soviet character of the Pilsudski regime. From Stalin's perspective, the Molotov-Ribbentrop Pact of 1939 only gave Russia its due in territory and the Poles' adamant insistence on the restoration of the Riga frontier became the major stumbling block in their relations with the principal Allies, for even the United States and Great Britain refrained from guaranteeing such a restoration. Although the Riga line had legal status and, for the Poles, great symbolic significance, it rested on shaky ethnic grounds, and Lukas might have considered its objective

merits instead of simply asserting its validity. In the event, Stalin prevailed. At Teheran Churchill proposed and Roosevelt acknowledged, to Stalin's great satisfaction, that Poland's eastern frontier be moved back to the 1919 Curzon line. The Poles, who had not been consulted, were furious but impotent.

As the war progressed, relations between the government-in-exile and the Allies turned from bad to worse. The United States and Britain squelched Sikorski's scheme for an East European federation because of its anti-Soviet overtones and deplored his public appeal for a Red Cross investigation of the Katyn massacre (indeed, Stalin seized the opportunity to sever relations with the London Poles and promote the Communist Lublin Committee as the core of a postwar regime). Roosevelt tried to pacify the London Poles and Polish-Americans with vague assurances that at war's end all would be resolved amicably. The American predilection for proclaiming idealistic war aims raised the expectations of the Poles and other East Europeans, but the gap between Wilsonian theory and political reality widened as Roosevelt concentrated on military strategy and postponed or left to Churchill the pursuit of contentious political issues. Stalin's patronage of the Lublin Committee and his ruthless hands-off response to the Warsaw Uprising of August-September 1944 left little doubt as to his designs on Poland, but Roosevelt's efforts at Yalta and after to secure a representative government for Poland were feeble and foredoomed. The outcome was deadlock, the fifth partition of Poland, and the imposition of a Communist regime.

Professor Lukas ably relates the details of this lamentable exercise in passive diplomacy. He contrasts the administration's distaste for the nationalistic and conservative Polish emigré leaders and for Polish-American lobbying with the official propaganda campaign depicting Stalin as a stalwart and politically respectable partner. To be sure, the Polish emigrés were at times unreasonably inflexible, and they paid a heavy price for taking the Atlantic Charter at face value. The author is restrained to a fault in his critique of Rooseveltian diplomacy. He even omits to note and comment on George Kennan's view that the Warsaw Uprising provided the opportunity for a showdown with Stalin over Eastern Europe. His account is at times elliptical, as in his passing reference to the arrest by the Russians in 1942 of Polish relief officials who had engaged in intelligence gathering activities (p. 24); the reader is left to wonder, what intelligence, and on whose behalf? But his study is well researched, with extensive documentation, and it stands as an accurate and straightforward account of relations

among allies whose strangest pair, in retrospect, were not the United States and Poland.

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LEE FINKLE. *Forum for Protest: The Black Press during World War II*. Madison, N.J.: Fairleigh Dickinson University Press. 1975. Pp. 249. \$12.50.

Lee Finkle's *Forum for Protest* begins by tracing the development of the black press, which persisted despite perennial financial difficulties, periodic white opposition, and internal ideological conflicts, from its inception in 1827 to the eve of World War II. Throughout this long history, Finkle shows that the press served black society well by protesting discrimination, publicizing philosophical differences, assisting black migrants, and providing leadership opportunities. He also contends that the press was assimilationist, democratic, and middle class in outlook, placing too much faith in American society. Against this background, Finkle analyzes the response of the press to World War II.

Drawing on an extensive study of the Chicago *Defender*, Pittsburgh *Courier*, Norfolk *Journal and Guide*, New York *Amsterdam News*, and Baltimore *Afro-American*, Finkle details how the press provided information about black servicemen, protested against discrimination in civilian and military life, and publicized the war's impact on nonwhite people world-wide. Like their counterparts in the Civil War and World War I, black editors, journalists, and columnists believed that the black community must participate in the war effort or risk being excluded from the peace. They also became sensitive to criticism, particularly from white liberals, leveled at their alleged inflammatory policies during the national crisis. Hence, Finkle argues, black newsmen shifted from an aggressive attack on discrimination at home to a crusade for civil rights by channeling the militancy of the masses into the war effort. With official United States entry into the Second World War, they adopted the Double V campaign—calling for victory at home and abroad—which inherently opposed radical methods, like draft resistance, as the means to achieving equality. Despite the stirrings of the masses and the militant rhetoric of their leaders, Finkle concludes that “the war years cannot be considered the beginning of the ‘black revolution.’” Rather they marked “the last effort of the old order” (p. 222).

Finkle is probably correct in suggesting that these newspapers were representative of others, although further research is needed on this point. He also shows that black morale was depressed by

the hypocrisy of the United States fighting fascism internationally, while practicing racism domestically; much more evidence is necessary, however, in order to equate depressed morale with racial militancy. As significantly, precise definitions are needed for “militancy” and, especially, “masses” before a complete interpretation can be made of black protest in World War II. Diversity within the black population must be evaluated before it can be assumed that the masses would have acted, and would have acted militantly, as a monolithic bloc.

Forum for Protest tells much about one of the most important institutions in the black community. Because many editors, reporters, and columnists (such as W. E. B. Du Bois, Adam Clayton Powell, Jr., and Walter White) were intellectual, political, religious, and civic leaders, Finkle's study goes beyond the press. Its major contribution, nevertheless, is in skillfully advancing a thoughtful thesis on attitudes and protest during World War II that challenges the standard interpretation but that itself needs additional documentation.

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MICHAEL R. BELKNAP. *Cold War Political Justice: The Smith Act, the Communist Party, and American Civil Liberties*. (Contributions in American History, number 66.) Westport, Conn.: Greenwood Press. 1977. Pp. xiv, 322. \$16.95.

This is a revision of a doctoral dissertation completed in 1973 at Wisconsin under Stanley Kutler. It is an in-depth study of the origins, uses, and effects of the 1940 Smith Act, the first peacetime sedition law since 1798, which the federal government vigorously employed against the American Communist Party after the Second World War. Admirably researched, judicious, and well written, this monograph is a significant contribution to the growing body of literature on the second Red Scare.

Michael R. Belknap writes from a moderately liberal perspective for the most part and sees the vaguely worded Smith Act as an attack upon the First Amendment. He is particularly critical of the FBI, the Truman administration, and the Vinson Court for championing a hysterical and in part politically motivated effort to crush a “potent radical movement.” He assigns the Smith Act prosecution of Communist Party leaders a more prominent role in the Red Scare than have other scholars, and he argues persuasively that the Justice Department's use of the legislation was primarily responsible for the virtual destruction of the party by the mid-1950s. The easing of international

tensions and the Warren Court get much of the credit for the "return to reason" that began in 1957.

One of the book's outstanding features is a lengthy and perceptive treatment of the 1949 trial of the Communist Eleven, presided over by Judge Harold Medina. The obvious parallels to the 1969 Chicago Seven trial should remind us of what can happen to American freedoms in times of national distress.

The author is unsure of himself only when he discusses the Truman administration's overall approach to the domestic subversion issue. He wants to disassociate himself from New Left interpretations and from Alan Harper's glowing tribute to the thirty-third president in *The Politics Of Loyalty* (1969), but he is unable to stake out a clear, consistent position of his own. This is ironic, for Belknap's exhaustive research in the Truman Library is responsible for much of his study's originality.

Despite the author's impressive knowledge of the American Communist Party, moreover, one might well question his thesis that the party was at all times essentially harmless, a mere whipping boy for zealous and cynical bureaucrats, politicians, and newspaper editors. Evidence to the contrary is abundant and goes virtually unexplored. This flaw may be traced in part to Belknap's almost uncritical acceptance at times of dubious historical recollections and interpretations emanating from the Far Left, including several fellow travelers and party leaders.

Readers will also notice that the author did not have access to confidential FBI documents. The Bureau demanded \$300,000 to screen relevant materials in its files, without an advance commitment to release anything. Belknap declined the offer, of course, but assures us that the files contain virtually nothing that cannot be gleaned from the Communist press. If he is correct, his book could well be the definitive study of the Smith Act. At any rate, it is important and deserves the serious attention of all students of postwar America.

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ALLEN WEINSTEIN. *Perjury: The Hiss-Chambers Case*. New York: Alfred A. Knopf. 1978. Pp. xxi, 674. \$15.00.

Historians of twentieth-century America, perhaps more than other Americans, are aware of the great symbolic importance which the Alger Hiss case has assumed in our recent past. Hiss's conviction in 1950 became an event that reinforced and fur-

ther stimulated the emerging "witch-hunt" and anti-Communist hysteria. The rightward political drift was strengthened as Joe McCarthy and others used Hiss as a convenient "proof" of how the architects of Yalta had betrayed their own nation.

In this first major study by a historian of the Hiss case, based on examination of recently released FBI files, as well as the Hiss defense team's own papers, Allen Weinstein concludes that "the body of available evidence proves that [Hiss] did in fact perjure himself when describing his secret dealings with Chambers, so that the jurors in the second trial made no mistake in finding Alger Hiss guilty as charged" (p. 565).

Alger Hiss, however, has consistently maintained from 1948 to this day that he was both innocent and the subject of a frame-up. Weinstein's book helps us to look at the facts anew and to face them squarely, no matter how troublesome that may be to New Deal liberals and radicals, both of whom have put so much symbolic importance on Hiss's innocence.

To this reviewer, it is virtually impossible to read Allen Weinstein's book and still maintain a firm belief in Alger Hiss's innocence. Weinstein cannot prove that Hiss did what Chambers said he had done, but he comes as close as possible. He has developed a picture of the Communist apparatus in Washington that effectively undermines much of Hiss's account of his own political past. Weinstein effectively proves the reliability of Whittaker Chambers's own description of the CP underground and his own role in it. He makes effective use of the letters sent by Josephine Herbst to her former husband John Herrmann, who had been part of that apparatus. Herbst's letters show that Herrmann, Chambers, and Harold Ware, head of the CP underground until his death, had all told Herbst that Chambers had met Hiss in 1934 and had at that time tried to recruit him to the underground. These Herbst revelations do not show that Alger Hiss engaged in espionage in 1938; they do establish that Hiss was active in Communist circles and that he knew Chambers in that milieu. Moreover, Weinstein shows that Herbst was not honest in her FBI deposition and that Hiss's defense chose not to put her on the witness stand.

Hiss and his defenders have raised the charge of forgery by typewriter. By careful and intricate detective work, Weinstein shows that the documents Chambers produced were typed on Hiss's old Woodstock and that Hiss lied to the FBI and to his own lawyers about the typewriter's whereabouts. While denying knowledge of the machine, Hiss had in fact given it to the son of his former maid in the spring of 1938. But he covered up knowledge of its postwar location until such time as he thought producing it would enhance his own credibility.

There was no FBI conspiracy to frame Hiss, Weinstein argues. Indeed, Weinstein depicts an inept and angry J. Edgar Hoover and, surprisingly, a cautious and always opportunistic Richard M. Nixon, who almost gave up on the case just at the very moment when it was to propel him to national attention.

At certain times, unfortunately, Weinstein uses material that is not persuasive. The resulting unnecessary overkill hurts his own case and has allowed critics to fasten on these in an attempt to undermine his entire work. That Andrei Gromyko told Edward Stettinius in September 1945 that he would be happy to see Hiss appointed temporary Secretary-General of the UNO may be "practically unique in the annals of Soviet-American diplomacy," but it is hardly germane to Hiss's activity in 1938 (p. 361). Much has been made of Weinstein's interview with Czech defector Karel Kaplan, who told him that when Noel Field was interrogated by Czech security in 1948 he named Hiss as "the other one involved" in State Department espionage. Field's words, if indeed he did utter them, have to be viewed cautiously, given the situation in which they were voiced (p. 201).

The weakest use of such material is undoubtedly Weinstein's interview with J. Peters, the fabled head of the entire CP underground in the 1930s. The Peters interview supposedly offers substantiation of Chambers's picture of the underground and, by implication, of his accusations about Hiss. But Peters, found by Weinstein in Budapest, evidently told him little, if anything. One has to turn to footnote ninety-five of the first chapter, to read Weinstein's explanation that "Peters smiled once during our talk when I suggested that his frequent use of the terms 'open' and 'secret' Communist Parties . . . indicated an awareness of that second realm which most Party 'functionaries' would deny having possessed" (p. 597). Weinstein has foolishly and unnecessarily sought to gain great mileage out of this now famous smile, a smile whose substance is an enigma.

Weinstein does write about the Cold War iconography around the case, but he fails to confront an issue raised by that iconography. If Hiss was guilty, as it is almost certain he was, it does not mean that Joe McCarthy or the Cold War liberals were correct in their postwar assessments. If Hiss was innocent, similarly, it certainly does not mean that other Communists or idealistic pro-Russian liberals did not decide to pass secrets to the Soviets in the 1930s. More important, Weinstein fails to stress that such actions in 1938 took place in a much different context than existed between 1948 and 1950, when espionage took on a quite different connotation.

Julian Wadleigh admitted that he had indeed

passed government documents to Chambers in the mid-1930s. But Wadleigh, in a newspaper article, sought to explain that he did so in the context of rising fascism and his frustration and anger at Western appeasement. He saw only the Soviet government coming to the aid of the embattled Spanish republic, and he offered his services to the Communists. "Technically," he wrote, "I was giving out some of the secrets of my own government to agents of a foreign country," but he stressed that he did not regard his action "as contrary to the interests of the United States." Indeed, Wadleigh thought he was acting in the American national interest, because he thought he was helping the Russians resist German and Japanese aggression.

One must ask: Was the type of material Wadleigh and Hiss passed to the Russians, pertaining to information about German and Japanese intentions abroad, truly harmful to American national security? Wadleigh and, most likely, Hiss did pass such documents. Hiss's troubles stemmed from his refusal to acknowledge that act. Had he done so, at one point he might have been able to avoid a perjury trial. But he chose a different course, affirming his complete innocence. Few will be able to believe his protestations any longer, if they allow themselves to honestly confront the arguments in Allen Weinstein's powerful book.

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Foreign Relations of the United States, 1951. Volume 6, *Asia and the Pacific*. In two parts. (Department of State Publication, number 8889 and 8918.) Washington, D.C.: Government Printing Office. 1977. Pp. xi, 1,477; x, 1,479-2,276.

This volume in the highly respected *Foreign Relations* series is a smorgasbord of American-Asian and American-Pacific relations. Despite the exclusion of documents relating to China and Korea, with 2233 pages of text volume six may well be the largest in the 1951 series. With many countries to cover, the number of pages per country necessarily is limited. Nevertheless, there is a more-than-adequate sampling of the issues that characterized American relations with the nations of Asia and the Pacific. Topics include: "East Asia and the Pacific" (265 pages), "Australia" (1 page), "Burma" (64 pages), "Indochina" (252 pages), "Indonesia" (194 pages), "Japan" (701 pages), "New Zealand" (12 pages), "the Philippines" (103 pages), "Thailand" (56 pages), and, in a general section on South Asia, "Multilateral Relations" (354 pages), "Afghanistan" (9 pages), "Ceylon"

(72 pages), "India" (118 pages), and "Pakistan" (30 pages). No review could do justice to such a wide-ranging volume.

Throughout the year 1951 and across the wide geographic expanse covered by the volume, the major issue was fear of Chinese Communist aggression. It is understandable that in the weeks and months after the massive Chinese intervention in Korea against United Nations forces that American diplomats believed it signaled the beginning of Chinese Communist aggression throughout Asia and the Pacific. From India to Indonesia a fear developed of Chinese "volunteers" taking advantage of resentment toward the European colonial powers to foment totalitarian government behind an attractive facade of nationalism. Thus American policy makers sought both to weigh the consequences of the loss of the region (with its hundreds of millions of people and its varied and vast resources) and to organize combinations of non-Communist governments. The enemy of my enemy is my friend—an old and respected diplomatic strategy—came to dominate policy. In the haste to embrace the domino theory—that the fall of any one country in the region (we had not yet settled on Vietnam) would cause the entire area to fall to Communism—one lone voice of reason was lost in the confusion. Charles Ogburn, a State Department officer, questioned policy that "conceived of prodigious outlays by the United States" and turned the cause of anti-Communism in Asia into an "American cause" (pp. 6–9). Sadly, his attempt to arrive at a more realistic policy largely was ignored. As the months passed, the commencing of truce talks in Korea had a somewhat unexpected result. Instead of lessening tensions along China's long periphery, it caused many American officials to fear that troops freed from fighting in Korea would see action as "volunteers" in South and Southeast Asia.

Despite a few typographical errors—some caught and some not—this volume will help illuminate our knowledge at a crucial time and place in modern American foreign relations.

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MARY FRANCES BERRY. *Stability, Security, and Continuity: Mr. Justice Burton and Decision-Making in the Supreme Court, 1945–1958*. (Contributions in Legal Studies, number 1.) Westport, Conn.: Greenwood Press. 1978. Pp. viii, 286. \$17.50.

Harold Hitz Burton, Truman's first appointee to the Supreme Court, maintained a record of how his colleagues voted on petitions for certiorari and

of their views in conference. He preserved this material, along with some intra-Court correspondence, and it is this material that provides the rationale for the present volume. Except for an introductory biographical sketch and a short summary chapter, the book traces, term-by-term, Burton's opinions and the cases decided by the Court. Apparently the author's purpose was two-fold: first, to provide a more detailed characterization of the justice; and, second, through Burton, to give us some insight into the Court from 1945 to 1958.

Labeling her subject "the quiet, unassuming model of the competent justice" (p. 234), Mary Frances Berry adds only some detail to the prevailing assessment of Burton. Comfortable in working within the boundaries of the status quo, the justice gained the respect of most of his colleagues and the recent law school graduates who, as clerks, served the Court. During an era of sharply warring personalities, Burton played the role of peacemaker, usually content to avoid the larger issues and decide cases on the narrowest of grounds. In a time of transition he supported the claims of blacks, but his insistence upon preserving the authority of the states left him behind as the Warren court reassessed the nature of the federal system. The image that Burton cultivated was that of a judge who had successfully divorced his personal views from his judicial labors. This image is not penetrated by Berry, though she does acknowledge his hostility to the tactics and demands of labor unions and his readiness to defend the institution of the family from attack.

Whatever value one wishes to place on a study of Burton's decision making, this book has more merit, not in its interpretation, which is analytically limited and tends to follow well-grooved paths, but in the conference material that it makes available. Although Burton's notes are far less revealing than those of Harlan Fiske Stone, they do add new information on the working relationships of the justices. We see the deliberative process at work and are encouraged that personal views can give way during it. We see Douglas wavering on a free speech claim and Frankfurter claiming that the underworked Vinson court was proceeding too hastily. There are no bombshells here, but the details provided illuminate the internal working of the Court.

Although the book suffers from a lack of consistently clear writing and an uncertain focus, its useful information is poorly served by the index. Since the material often tells us more about the other justices than about Burton himself, it is unfortunate that Frankfurter and Black, who figure prominently in the text, are not indexed after the first five pages. The sparse references that follow

significant entries seem arbitrarily chosen. Such a misleading index is worse than none at all.

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LYNDA ANN EWEN. *Corporate Power and Urban Crisis in Detroit*. Princeton: Princeton University Press. 1978. Pp. viii, 312. \$17.50.

Lynda Ann Ewen offers *Corporate Power and Urban Crisis in Detroit* as an explicitly "Marxist-Leninist" analysis designed to provide the "working class" with "knowledge to protect its class interests" (p. 292). But since she also seeks to contribute "a disciplined and systematic analysis" (p. v), presents her study through a distinguished university press, and makes much use of work by historians and sociologists, her book calls for scholarly review as well as political debate.

Corporate Power and Urban Crisis makes one contribution which many readers will find useful—an extensive study of the corporate and civic positions held by the 421 persons who directed forty-one of the largest industrial, banking, retailing, and utility firms which had headquarters in the Detroit area in 1970. In a series of painstakingly gathered, though not always clearly presented, statistics, Ewen discusses patterns of family ownership, interlocking directorates, and corporate director representation on the boards of local cultural, educational, civic, and business organizations.

According to Ewen, these directors and their families, especially those who own stock in the firms they direct, constitute the Detroit region's "ruling class." Dominant at least since the American takeover of Detroit in 1796, this class, Ewen asserts, has controlled the region, making certain that profits have remained high, that its members have enjoyed an attractive physical environment, and that labor has remained exploited, impoverished, weak, and divided along ethnic and racial lines. Black militancy and the energy crisis have recently threatened the ruling class, but it plans, Ewen believes, to respond to these and other problems by introducing fascism. On the basis of this analysis, Ewen calls on the working class to create a "United Front Against Fascism."

In its historical parts no less than in its discussion of the present, *Corporate Power and Urban Crisis* is exceedingly schematic. Ewen uses several works of the ancestor-glorification genre to develop a "history that indicts the ancestors" (p. 47). But her exclusive reliance on such sources and her neglect of historical scholarship on urban elites severely limit the value of this indictment. Thus

she gives an account of certain long-lived families but not of Detroit's early landowners, merchants, and manufacturers as a group, and she neglects the impact of automobile manufacturing, save for an assertion that "the ruling class incorporates newcomers within its web" (p. 75). She also does not indicate what proportion of the ruling class of 1970 descended from wealthy Detroit families of the past. Her discussion of power is even less satisfactory. While she recognizes that her work "would be greatly strengthened by data on the specific processes by which the ruling class actually influences and controls" civic organizations and corporations (p. 209), she offers little evidence on these points, and still less for her view that the ruling class has long controlled local and state politics. Instead, she presents highly charged interpretations of several local policies and events. Her history of labor in Detroit suffers from similar faults.

Though Ewen does not say so, much of her work seems designed to engage the members of Detroit's recent League of Revolutionary Black Workers, described in James A. Geschwender's *Class, Race, and Worker Insurgency*. But whatever its role in political debate, *Corporate Power and Urban Crisis* will convince few readers who do not share its author's interpretation of the "fundamental scientific laws observed and developed by Marx" and "developed and expanded by such men as Lenin, Stalin, Dimitroff, and Mao Tse-Tung" (p. 7). Marxist readers no less than others will be surprised to find so many controversial issues settled by reference to the polemics of the 1930s. Historians of all persuasions will regret Ewen's decision to substitute schematic assertion for the serious study of the past and the present.

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BRUCE M. STAVE. *The Making of Urban History: Historiography Through Oral History*. Beverly Hills, Calif.: Sage Publications. 1977. Pp. 336. Cloth \$15.95, paper \$6.95.

When the *Journal of Urban History* began to appear in 1974, it carried a unique feature: almost every issue featured an extensive interview with a senior urban historian discussing his or her background, development, and work, and assessing recent scholarship. Now Bruce M. Stave, who conducted the interviews, has presented in one volume his conversations with Blake McKelvey, Bayrd Still, Constance McLaughlin Green, Oscar Handlin, Richard Wade, Sam Warner, Stephan Thernstrom, Eric Lampard, and Samuel Hays. As inter-

esting as the individual interviews may have been, it is pleasant to discover that in this collection the whole is considerably more than the sum of the parts.

There are several reasons for this. Stave has added thorough annotation, references, and an engaging introduction, all of which make the volume extraordinarily useful for students of urban history. And, in a paradoxical sense, the collection is more valuable than the isolated interviews because it ends up being less historiographical, rather than more.

This reviewer was perhaps not alone in sensing a strained quality in the original interviews, as if the ambitious new *Journal*, by including them, was seeking to provide a not quite legitimate sub-discipline with a self-generated and self-validating historiographical tradition. The weakest moments in the interviews generally occurred when Stave asked his subjects how and why they became urban historians or how they assess the future of urban history. This disrupted the flow of more wide-ranging discussions, and often seems to have drawn more embarrassment than enlightenment from the subjects.

The collection shows this disciplinary self-consciousness to be less problematic and more peripheral than it seemed in the *Journal*. In fact, Stave might well have reversed the elements of his subtitle, for in the collection historiography seems merely the frame, and a deeper oral history emerges as the book's most important product. The book is superb oral history in several senses. It provides an intellectual history of a remarkable generation of scholars, who started from a striking variety of backgrounds and made their way along different tracks through the obstacles of the 1930s, 40s, and 50s, improbably arriving at shared interests and concerns. Secondly, it demonstrates the value of encountering such material in the first person, where the intersection of biography, history, and personality can be studied under a kind of natural illumination. Finally, there is the special pleasure to be derived from sharing the enthusiasm, the depth of insight, and the breadth of knowledge with which these senior figures discuss their work and their profession.

Much of this quality can be credited to Stave, who seems to have been a superb interviewer, adept at giving his subjects plenty of room, yet helping them to focus their remarks through well-placed asides and follow-up questions. He also should be praised as an editor for allowing us to see those occasional false starts and missteps when the balloon of conversation suddenly deflates; these provide useful insights into the skill of interviewing itself.

But beyond Stave's evident mastery of this skill,

the book's interest and quality are ultimately a matter, properly, of the people whose remarkable self-portraits are presented. Inspiring as individuals, these historians become especially impressive when encountered in a group representing a scholarly period at its best and in a form that permits us to appreciate the vitality of their lives, work, and thought. There is much to be learned here about urban historiography, but, as the book speaks more deeply about history and historical consciousness in the broadest sense, it should be of value to those outside a circle of specialists, and it will help as well to lift the vision of those within.

MICHAEL FRISCH
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Buffalo

THOMAS BENDER. *Community and Social Change in America*. (Clarke A. Sanford-Armand G. Erpf Lecture Series on Local Government and Community Life.) New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xiii, 159. \$8.00.

In the first part of this lively and provocative book, Thomas Bender offers a stunning critique of the definition of community common in twentieth-century social theory. Misusing Ferdinand Tönnies, social theorists have defined community solely in terms of locality, ignoring a second and equally important component of the original concept, namely, its delineation of a style of relationships. In Bender's words, community is "a social network characterized by a distinctive kind of human interaction" (p. 11). Properly understood, community never disappeared. Rather, its manifestations have shifted, and it is the complex tension created by changing configurations of communal and noncommunal elements of social experience that should form the focus of research.

Bender contends that American historians have been trapped by notions of communal collapse, which, he asserts, is the metaphor underlying much of the most creative and well-known scholarship of the last several years. Here he is less convincing because he confounds, under one overly general rubric, the differing descriptions of social experience offered by these historians and transforms their attempt to analyze—often with precision—the impact of urbanization and industrialization into a simplistic notion of communal decay.

To counteract what he takes to be the common interpretation, Bender offers a hypothetical history of community in America that is insightful, although more a modification than a new reading of the past. His demonstration in another chapter of the potential of network theory for historical

research, however, is fresh and convincing. Similarly, he manages to draw out the implications of his work in a way that puts a useful, new perspective upon questions of community in contemporary policy.

As attested by his footnotes, Bender's work draws on a deep, impressive mastery of the literature in a variety of disciplines. It is characteristically graceful and intelligent throughout. A few problems, nonetheless, should be noted. First, the distinction between communal and familial styles of social relations is not made, although by implication Bender does consider them distinct, as indeed they must be if his notion of community is to be anything other than family writ large. Second, although he introduces a market economy as an important factor in the Gilded Age, Bender virtually ignores the impact of wage labor upon the transformation of social relations. The spread of wage labor, however, offers a powerful explanation of the change in social relationships that he describes. Third, the process of social change he portrays is dialectical, a point he makes only in passing. A more explicit delineation of the dialectic of community, however, would have shown more clearly the sources of tension and change.

Finally, Bender emphasizes the importance of evaluating social relations in public and private life with different standards. It makes no sense, he rightly says, to look for community where it cannot exist. But his dichotomy is too sharp. It is possible to transform radically some public tasks so that they may be carried out in a more communal style. The debate should focus on the desirability of that goal and the means of its implementation. The danger with Bender's argument is that it could be used as a sophisticated justification for the preservation of the status quo, a usage with which he surely would not be pleased.

MICHAEL B. KATZ
University of Pennsylvania

ELEANORA W. SCHOENEBAUM, editor. *Political Profiles: The Eisenhower Years*. New York: Facts on File. 1977. Pp. xx, 757. \$45.00.

NELSON LICHTENSTEIN, editor. *Political Profiles: The Kennedy Years*. New York: Facts on File. 1976. Pp. xvi, 621. \$45.00.

NELSON LICHTENSTEIN, editor. *Political Profiles: The Johnson Years*. New York: Facts on File. 1976. Pp. xvi, 741. \$45.00.

Although the editors disclaim any intention of making the *Political Profiles* series into a collective biography of political elites, students of recent

American history will find that it resembles one. These three volumes are the first of six that will span the years since 1945 and fill part of the gap not covered by other biographical reference works. With the exception of the proposed Nixon/Ford volume, each book covers a single administration and summarizes a particular phase in a person's political career. Yet each volume is also a part of the whole, and the biographies will evolve with the series. The principal focus is on national political figures, but secondary elites and state and local leaders are not ignored. For example, the Johnson volume, like others in the series, includes administration officials, some military officers, most Senators, and many members of the House of Representatives. But the Johnson volume also includes nearly four dozen prominent blacks and leaders of the antiwar movement and the New Left. The Truman volume will cover more trade union leaders, and the Nixon/Ford volume will have more radicals, women, and minority representatives than the Eisenhower volume.

Most readers will probably disagree with some of the selections. An (all male) Editorial Advisory Board of recognized historians and journalists assisted in the selection process, and their criteria emphasized the degree of political participation and the corresponding public recognition of the persons considered. Thus, they selected a few Rockefellers and rejected the Mellon and DuPont families. The editors provided twelve- to twenty-page introductions to establish the political atmosphere of each administration and added a spotty but useful bibliography. The "Appendix" is a handy reference to a chronology of events and to lists of administration officials, members of Congress, and governors of the states during the period covered.

The contributing editors, who were primarily doctoral candidates or new Ph.D.s in history, produced between 500 and 600 profiles for each of the volumes. The pages are printed in double columns with the profiles varying in length from one column to several pages and usually—but not always—reflecting the relative importance of the subject. While some of the profiles are superficial and uneven in style and content, others approach the level of the biographies in the *Dictionary of American Biography*. Interpretation is not eschewed, and the profiles sampled by the reviewer are generally accurate and incorporate current scholarship. Multiple authorship and the sequential arrangement of the series have caused some stylistic anomalies and considerable repetition. Profiles of Robert Kennedy, which range in length from three columns in the Eisenhower volume to six and one-half columns in the Kennedy volume and five in the Johnson volume, were written by separate au-

thors, and differences in style and emphasis are apparent.

The chronological arrangement has distinct advantages, but it makes some repetition unavoidable. In addition, the editors declare that the set will contain more than 2,500 profiles when completed, but it appears that the total number of individuals covered might be smaller. In the three volumes under review, more than 240 persons were included in two and approximately 160 were included in all three volumes, thus reducing the nearly 1,600 profiles to an actual body count closer to 1,200. One of the principal dilemmas in studying recent history is weighing potential historical significance against contemporary notions of newsworthiness or notoriety. The editors did not resolve the dilemma. Walter Lippmann, who is included in the Eisenhower and Kennedy volumes, is absent from the Johnson volume, and I. F. Stone is in all three. Henry Kissinger is included in the Eisenhower and Johnson volumes but not in the Kennedy volume, while Richard C. Lee, mayor of New Haven, Connecticut, and Alex Rose of the Liberal Party of New York are covered in all three volumes.

The editors consciously avoided quotas in order to choose the persons who were most prominent in public affairs. This meant the inclusion of a significant number of blacks, but there are very few women and almost no representatives of other racial minorities in these three volumes. The result undoubtedly suggests the dominant trends in American political and social life during these years, but it also reinforces the relative Eastern and urban tilt of the volumes.

Numerous typographical errors are annoying. The books are expensive enough to warrant good printing and careful editing. While there is some irony in spelling Alger as "Anger" Hiss, listing Republican Congressman John H. Rousselot as a Democrat in the "Career Index" contradicts the correct listing of his political affiliation in his profile. These criticisms aside, teachers will find the series helpful in preparing lectures, reference librarians should welcome it, and students will find it informative. That such a series appears to be both necessary and useful serves as a comment on changing historical methods and the concern with the role of elites in American politics.

ECKARD V. TOY, JR.
University of Oregon

JULIE ROY JEFFREY. *Education For Children of the Poor: A Study of the Origins and Implementation of the Elementary and Secondary Education Act of 1965*. Columbus: Ohio State University Press. 1978. Pp. xiii, 270. \$15.00.

In this study, Julie Roy Jeffrey contends that the Elementary and Secondary Education Act of 1965 failed for many reasons. First, policy makers did not examine their own fundamental beliefs about the causes of poverty. Second, the possibility that poverty was not solvable by education was never considered. Third, political compromise bargained away any possibility of an effective program. Jeffrey surveys federal attempts at educational reform legislation in the 1950s, the circumstances that surrounded passage of the act in 1965, and the implementation and demise of the act, along with a catalogue of lessons for policy makers. The strengths of this thesis lie in Jeffrey's research in congressional testimony and in secondary materials.

The work, however, is flawed in two major respects: first, its poor command and use of educational history; and, second, Jeffrey's failure to question her own assumptions about the role of social scientists in educational reform politics. The first flaw appears frequently. For example, Jeffrey concludes that the parochial school issue was a false one because, in the end, parochial school children did not materially benefit from the act. This ignores the meaning of the separation of church and state in American educational history throughout the twentieth century. The church-state issue shaped the legislative compromise that ensured limited aid to parochial school children in the first place; so the numbers of parochial school children receiving aid says nothing one way or the other about the significance of the church-state issue. Furthermore, the compromise "child benefit" theory was not created by the educational bureaucracy as Jeffrey implies but derived from the U.S. Supreme Court's opinion in *Cochran v. Louisiana* (1930). Lyndon Johnson built his coalition for passage of the act around this constitutional principle.

Jeffrey discusses educational reform in urban centers as if it were unique to the 1960s. She criticizes Job Corps centers for dealing with only "character-building and training" and for not concentrating "on developing an approach to education that might succeed with the poor" (p. 49). Had she considered the history of educating America's urban poor she would have realized that those were precisely the goals of the vocational schools in the early decades of this century. Similarly, had she been aware of the parent education programs of the 1920s she would not have found similar programs in the 1970s so new and promising.

Aside from the historical problem, there is a difficulty in the author's failure to challenge her own assumptions. For example, running through the work is the notion that planners never ques-

tioned whether education would end poverty. Yet Jeffrey does not examine what the implications would be if unemployment and poverty were endemic to the economic system. At one moment she generalizes that "education, despite popular beliefs, had not and could not solve major social problems" (p. 220), and yet she argues that more research, more funds, more controls, more radical restructuring of schools might teach the poor to perform like middle-class students. The culprits in all this, according to Jeffrey, are local school boards and school systems, which she repeatedly describes as inflexible and uninterested in the problems of the poor. This is both unfair and inconsistent. After pointing out that political compromises made to ensure passage guaranteed educational failure and mismanagement, she cannot then indict school boards and intransigent school administrators for the lack of success.

Perhaps the most striking weakness is Jeffrey's failure to subject social science researchers to critical scrutiny. She repeatedly implies that the real problem was that pragmatic politics interfered with academic social scientists and their search for objective truth. The failure to recognize that social science experts are political actors in a political process serving a political constituency fundamentally flaws this work.

CLARENCE J. KARIER
*University of Illinois,
Urbana-Champaign*

BARTON C. HACKER and JAMES M. GRIMWOOD. *On the Shoulders of Titans: A History of Project Gemini*. Washington, D. C.: National Aeronautics and Space Administration. 1977. Pp. xx, 625. \$19.00.

The Gemini program conducted by the National Aeronautics and Space Administration (NASA) between 1961 and 1966 has suffered from its position between the nation's first adventures in space with Mercury and the more dramatic lunar landings of Apollo. But the twelve loftings of Gemini constituted a vital bridge between the initial hesitant parabolic probe of Mercury and ultimate success with Apollo in placing men on the moon. This excellent volume by two NASA historians details this role of Gemini with its succession of trials and triumphs, including the first rendezvous of one spacecraft with another, maneuvering in space under the control of astronauts rather than ground stations, flights in excess of a week, forays outside the spacecraft by astronauts, and precision reentries that landed within sight of waiting carriers. No less important were the physiological and psychological barriers surmounted when Gemini flights demonstrated that men could pass through

the earth's radiation belts without serious harm and perform demanding tasks even when subjected to prolonged weightlessness. Without these steppingstones, a successful first try lunar landing would have been improbable.

The focal point of this study is the Gemini Program Office at what is now the Johnson Space Center outside Houston, Texas. GPO monitored a complex series of projects in developing the spacecraft itself—such ancillary equipment as fuel cells, self-contained life support space suits, and a para-glider for use in the final descent. The latter is worth mentioning in particular because it represents a road not taken after extended trial. Seen in retrospect, there is always a danger of conceiving success as a straight line of development, each step a logical outgrowth of the one before. The authors are at pains to bring out what actually transpired, the groping, the decisions made with limited information, and the recalcitrance of novel items that failed to perform as intended despite lavish expense. Above all, they show how planning is inescapably opportunistic, evolving pragmatically as successful experiment reveals possibilities, sometimes justifying the omission of a previously planned sequence of steps, sometimes requiring an unanticipated digression and outlay of funds when equipment or procedures fail.

The problem of funding is treated with particular insight. To secure appropriations from a skeptical Congress, NASA administrators had to provide a rationale related to large and salable goals such as staying ahead of the Russians or being first on the moon. But often the precise relation to these goals of a particular scientific experiment such as ion wake measurement or frog egg growth studies (for assessing possible genetic damage) proved difficult to establish convincingly. Trying to provide that extra margin of safety where human lives are at stake tended to escalate costs and delay schedules, much to the concern of Congress. NASA leaders, therefore, were repeatedly forced to decide how much testing was enough. Reliance on the Air Force Titan for a booster gave the Department of Defense, under Secretary McNamara, an opening to seek control over the program. This move, treated by the authors from a NASA perspective, illustrates still further the impressive scope of problems confronting NASA decision makers.

Historians of technology and students of government in an era of high technology will welcome this valuable study. There is a minimum of jargon; the authors define all acronyms and their explanations of technicalities are for the most part lucid. Their treatment of in-flight malfunction analysis by teams of engineers and scientists at the control center is downright exciting. On the negative side, too many personalities march on stage

without adequate introduction, the differing contractual philosophies of the air force and NASA are not effectively explained, and the contractors' side of the story is seriously scanted.

I. B. HOLLEY, JR.
National Defense University

PETER BRAESTRUP. *Big Story: How the American Press and Television Reported and Interpreted the Crisis of Tet 1968 in Vietnam and Washington*. In two volumes. Boulder, Colo.: Westview Press. 1977. Pp. xxxvii, 740; x, 706.

Although the Tet offensive occupied but a few weeks in the endless Vietnam War, it constituted a turning point in both the military action and in the status of the war as a domestic political issue. As a result, Tet has become something of a critical case in attempting to assess the effect of news reporting on American politics in general and the outcome of the Vietnam war in particular. The Tet offensive had been preceded by a "progress campaign" by President Johnson in the fall of 1967: light was visible from the end of the tunnel, the pacification program was working, the ARVN troops were fighting more effectively, and the country was more secure. But in late January 1968 the Viet Cong brought the war into the cities, particularly Hue and Saigon, in a nine-week burst of savagery. The episode was capped by a siege of the beleaguered outpost at Khe Sahn that was reminiscent of World War II. The events, as reported, seemed to call into question all of the administration's predictions concerning the war and made hundreds of official statements, press conferences, and briefings appear as a tissue of lies. The message was disaster; the war was lost or could not be won. Following Tet one could detect in the television commentators, particularly Walter Cronkite, a decisive shift of sentiment against our engagement. If Vietnam was a living-room war, this aspect of the war was first apparent during the Tet offensive, and the support and resolve of the American public seemed to dissolve in its wake. During and after this episode American politics started to unravel: McCarthy entered the primaries, Johnson announced that he would not seek a second term, and Robert Kennedy was assassinated.

Yet, if the above is true, we are faced with an anomaly. First of all, the Tet offensive was not a surprise; it had been predicted rather regularly by Westmoreland and the American military. It was not a military victory for the enemy; the offensive failed in its major military objectives and dealt massive casualties to the Viet Cong, dissolving many of the crack military forces of the insurgency for a year. Some writers have insisted that misreporting of this event was the critical episode in

Vietnam. The war was lost not on but by the media.

Peter Braestrup, a reporter for both the *New York Times* and *Washington Post* during the Vietnam years, one of the most respected of the correspondents covering the war, has attempted to sort out the press and television coverage of Tet and reach a judgment. How did the news media interpret the Tet offensive, and what effect did this have on the war and on the American public? The results is two massive volumes, one of text and the other of documents, that have now, mercifully, been reduced to a one-volume paperback by Anchor/Doubleday.

Braestrup's judgment, stripped to essentials, is this: the American press did perform badly during the Tet period. It was an event in which all the typical habits of the press—how stories are covered, pursued, written about, and so forth—produced both a confused and misleading account of events in Vietnam. Partly because of cynicism and distrust of the military and the Johnson administration, the press did not take seriously the warning that the offensive was coming. Because the attack hit Saigon where reporters were accustomed to living in safety and because it breached, if only symbolically, the walls of the American embassy, the success of the attack was much overplayed. And, more to the point, when the truer story started to emerge in the weeks following the first attack, there were few systematic efforts in the media at correction and clarification. Because the siege at Khe Sahn had the comfortable elements of traditional and pictorial drama, its military importance was overdrawn.

Braestrup is careful to point out that these errors and insufficiencies did not derive from any ideological opposition of newsmen to the war effort or to official deception. Newsmen generally supported the war, and during Tet the military information system functioned as well as it ever did. Rather, the increasingly hostile relations between the press and administration and, most importantly, the inability of normal journalistic practice to cope with ambiguous and rapidly changing events triggered the distortion. Yet the events themselves and the reporting did not have, or so it seems, the effect others have indicated on American public opinion on the war. Rather, it appeared that the Johnson administration was also taken in by the news reporting, and the confusing response of the administration led to that most dangerous image in presidential politics: a president not in control. American presidents can take massive difficulties and setbacks; indeed, under such circumstances the public will rally around them. But they cannot appear to be confused and indecisive or their support will melt.

Braestrup has not quite sustained his argument,

and he has not had the last word on these events, but his thorough and wide-ranging book is important for the quality of the evidence and the singular importance of the event both for the history of the Vietnam War and for the general problem of interpreting the effect of the press on American politics.

JAMES W. CAREY
University of Iowa

ARTHUR M. SCHLESINGER, JR. *Robert Kennedy and His Times*. Boston: Houghton Mifflin Company. 1978. Pp. xvii, 1066. \$19.95.

This superbly written book does not pretend to be objective. Arthur M. Schlesinger, jr., a close friend and political ally of the Kennedys, had access to family papers and to oral history memoirs from Kennedy admirers. He describes Robert Kennedy as "the most creative man in American public life when he was killed" (p. xiii) and as a "representative man" in Emerson's phrase—one who embodies the consciousness of an epoch, who perceives things in fresh lights and new connections, who exhibits unsuspected possibilities of purpose and action to his contemporaries" (p. xi). Pursuing this theme, Schlesinger employs a "life and times" approach in a book that he must have been tempted to label "The Age of Kennedy."

In seeking to avoid hero worship, Schlesinger occasionally portrays the "bad Bobby." The young Kennedy, he emphasizes, had to struggle against feelings of physical and intellectual inadequacy and to prove himself to a very demanding father. This effort made him appear cold and ruthless. His father, indeed, bragged that Bob was "hard as nails" (p. 97). Ted Sorenson added that Bobby (in the 1950s) was "militant, aggressive, intolerant, opinionated, somewhat shallow in his convictions . . . more like his father than like his brother" (p. 109). The novelist William Styron later reminded historians that Kennedy could "put people off horribly" (p. 815). Schlesinger himself concedes that Kennedy was slow to develop sensitivity regarding civil liberties and civil rights, that he abetted plots against Castro's life, and that he was too cautious in opposing Lyndon Johnson in 1967-68.

But Schlesinger regularly accentuates the positive side of Kennedy. Stressing Bobby's capacity for growth, he approvingly cites Richard Goodwin, who said that Bobby made "the greatest contribution to the success and historical reputation of his [John Kennedy's] Administration" (p. 599). Schlesinger tries especially hard to refute the impression that Kennedy was ruthless. Here he quotes Kenneth O'Donnell and David Powers,

who described Kennedy as the "kindest man we ever knew" (p. 594). The tag of ruthlessness, Schlesinger insists, was not only inaccurate but historically damaging: it led Kennedy to become almost obsessed with proving that it was not true. For this reason, Schlesinger says, Kennedy moved more gingerly than he should have in confronting Lyndon Johnson.

Many readers may feel that Schlesinger works too hard to praise Kennedy's friends and especially to diminish almost everyone else. His portrayals of men like Hubert Humphrey and Adlai Stevenson are unflattering; his picture of Johnson is witheringly one-sided. Schlesinger only grudgingly describes the bitterness that Eugene McCarthy and his followers felt toward Kennedy in 1968. This is essentially Manichean history: Kennedys as heroes, others as villains or fools.

In relying so heavily on oral history testimonials, Schlesinger fails to dispose satisfactorily of the charges that Kennedy was ruthless and opportunistic. This is not to say that Schlesinger suppresses evidence for such charges (though he virtually ignores the pre-convention presidential campaign of 1960, when the image of ruthlessness acquired a clear focus). Rather, it is to say that many readers will be unable to understand why much of America feared or hated Robert Kennedy. When Stevenson observed in 1964 that "the avarice of the Kennedys really makes me sick" (p. 667), he expressed a widespread sentiment that Schlesinger's arguments do not fully account for.

Schlesinger's "representative man" thesis also fails to persuade. Kennedy's presidential campaign perhaps represented the yearnings of underprivileged groups. But his candidacy lasted less than three months. Moreover, Schlesinger eschews the kind of voting analysis that might have outlined the sources of Kennedy's appeal. For whom, then, and when, was Kennedy the representative figure of his era? Given the deep divisions of the 1960s and the fears that Kennedy aroused at the time, it is unlikely that any one person—let alone a politician who never became a presidential nominee—could capture an epoch. Seeking such "representative men" is, in any event, a dubious exercise for historians writing about a nation as heterogeneous as the United States.

Fortunately, the book may be read without worrying about whether Kennedy was such a representative. Readers who ignore that argument, and who remind themselves of Schlesinger's partisanship, will find a richly documented account of events as Kennedy people perceived them. And because Schlesinger is a master writer, they are likely to be absorbed by all 916 pages of narrative. Other politicians should be so lucky.

JAMES T. PATTERSON
Brown University

CANADA

ROY MACLAREN. *Canadians on the Nile, 1882-1898: Being the Adventures of the Voyageurs on the Khartoum Relief Expedition and Other Exploits*. Vancouver: University of British Columbia Press. 1978. Pp. xix, 184. \$14.95.

The 1884 Wolseley expedition, belatedly authorized by the Gladstone government to rescue Gordon in Khartoum, faced a serious logistical problem in getting troops and supplies from the railhead up the cataracts of the Nile. With memories of his earlier expedition to Red River (Manitoba), Wolseley requested a force of Canadian "voyageurs," and 386 were dispatched quickly for these paramilitary duties. Few voyageurs of the fur trade were still alive, so recruits were drawn mainly from the raft drivers of the lumber industry. In this slender volume, Roy MacLaren, a Toronto businessman, presents the dramatic story of the exploits of the voyageurs and a few other Canadians who happened to be in the area. The book is popular history written with some flair but more concerned with colorful detail than historical analysis.

With the exception of a few manuscript sources in Toronto and Ottawa, the volume is written from secondary sources, especially C. P. Stacey's *The Nile Voyageurs 1884-85*. The work provides some new detail but no change in our basic knowledge of the role of the force. They did useful work in speeding the progress of the expedition and probably saved lives. This cannot disguise the fact that the expedition arrived too late to influence Gordon's fate. In spite of Queen Victoria's enthusiasm for this exotic group of backwoods Canadians, the significance of the voyageurs for imperial history is limited and strictly negative. When the British reconquered the Sudan thirteen years later, the military had learned the lesson of logistical supply and built a railway rather than depend upon the river.

MacLaren gets into difficulty when he tries to assess the significance of the boatmen for Canadian history. He concludes that their action "established a precedent" for later military contributions to imperial wars (p. 130). The evidence in the Macdonald and the Laurier papers shows that both prime ministers rejected any such interpretation. The author's second major claim involves the boatmen's contribution in developing a "common spirit" of nationhood in Canada. He writes that "the great Sudan expedition in which the French-speaking and English-speaking Canadians had worked side by side became an event in the gradual evolution of Canadian nationhood" (p. 130). But he fails to mention that in 1885,

French- and English-speaking Canadians were furiously divided by Riel's agitation in the West, and after 1885 imperial relations intensified the divisions between the English and the French in Canada.

Small topics can make fascinating books when the authors apply probing analysis to their subjects. Here too many central questions are left unanswered. If the imperial enthusiasm was so intense, why did three-quarters of the boatmen return to Canada with Gordon's fate hanging in the balance? How did these events contribute to the curious sub-imperialism of the Dominions? What were the social relations within the work force among English, French, and Indian Canadians? The reader is left to ponder these and many more. Also, one has to protest the Victorian stereotype of the Mahdi as only a "religious fanatic" (p. 131). There are many interesting events recounted in this volume but too little analysis of their historical significance for Canada.

R. J. D. PAGE
Trent University

LATIN AMERICA

KENNETH R. ANDREWS. *The Spanish Caribbean: Trade and Plunder, 1530-1630*. New Haven: Yale University Press. 1978. Pp. xi, 267. \$17.50.

Kenneth R. Andrews makes two assumptions in this work: one, that the propellant of history in the Caribbean during the years 1530-1630 was the northern drive for trade and plunder and, two, that every historical change in one place of the Caribbean region and the islands tended to affect, directly or indirectly, immediately or after some delay, most other parts of the area. The important innovation of the book is to give Spain an equal voice; the author makes extensive use of Spanish records in order to tell the story of the Caribbean when it was still a Spanish lake. Although Andrews modestly disclaims that he is writing a history of the region for the period, he has in fact written the best comprehensive account available.

The book has no bibliography, but the notes indicate plainly its documentary base and contemporary reference. A. P. Newton is acknowledged as precursor, and Irene Wright, Sluiter, Morales Padrón, Demorizi, and Incháustegui—all of whom have pioneered work on some aspect of Spanish Caribbean sources—are frequently cited. Archival research—at the *Archivo General de Indias*, Simancas, the *Museo Naval* in Madrid, and other Spanish depositories—has resulted in telling passages. Andrews's English references profit from his ex-

tensive work on maritime history, published to much acclaim over the years.

At the start of this book, settlement of the Greater Antilles and points along the shore had already passed the first stage of colonial development. The history of this area is the change of settlement patterns, the endurance of harbors despite raids, the disappearance of towns despite support, and of the claims, staked or made good, to islands or coastal towns by various European nations. The author discusses the causes for such development with reference to European politics, which determined the entrepreneurship of merchants, bankers, shipowners, and mariners. Spanish colonial interests, he argues, reflected the demand and supply of a considerable hinterland, from northern New Spain to Manila and Peru. This fact is illustrated by the French use of "Le Pérou" to refer both to the island of Española and to the whole Caribbean during the sixteenth century.

By 1530 the demise of Caribbean natives was progressing so rapidly that the social role of the exploited fell to imported slaves, and the Cimarrones came to constitute the local threat from below. The part played by that rebel population emerges in this account as more important, ubiquitous, and persistent than it has hitherto been presented. In a chapter on defense, Andrews explains the choices made in Spain between convoys, coastal defenses, and even evacuation of a coastline to strengthen security. Accounting for the change over time of Hawkins's career, Andrews reiterates the central point of the book—that this history must be seen not so much in black and white but as a web of interlocking interests and local variations. Although this approach precludes generalization, it does not invalidate explanation and comprehension. The chapters on "Contraband and Crisis," "The Spanish Response," and "Northern Settlement" reveal Andrews's exceptional control over masses of material.

Still to be heard from are the continental Americans to whom the Caribbean was not a rounded sea but a moving frontier, part of the Atlantic coastline. Such a concept would have been more familiar to Drake than that of the Caribbean as an "American Mediterranean" (p. 2).

This book nonetheless represents a major step forward toward a comprehension of the Oceanic frontier of Hispanic America.

URSULA LAMB
University of Arizona

MARIO LLERENA. *The Unsuspected Revolution: The Birth and Rise of Castroism*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 324. \$12.50.

In his foreword to this book the English historian Hugh Thomas notes that he read an early version when he was preparing his own work, *Cuba: The Pursuit of Freedom*. Thomas states that Mario Llerena's study "struck me then as an exceptionally clear and honest account . . ." and that he considers it "a first-class source on the revolutionary years of the 1950s" (p. 12). This reviewer agrees. Llerena's book presents the best argument I have read for the "revolution betrayed" thesis.

Other Cuban exiles have written about their experiences with the Castro movement and have argued that Fidel Castro betrayed the revolution they had supported. Llerena, however, has much better credentials than the others to make this argument. He joined the Castro organization in June 1956 after participating in various anti-Batista activities. For a time he believed that the 26th of July Movement offered the best vehicle to overthrow Fulgencio Batista and to build a democratic, humane society based on the Constitution of 1940. Ironically, Llerena feared at one point that Castro might make a deal with the oldline politicians and simply restore "politics as usual" after victory. After all, Castro's reputation as a former gang leader at the University of Havana was well known. But, after meeting with Castro in September 1956, Llerena's reservations were "somewhat dispelled" (p. 88).

Llerena was finally forced to leave Cuba in mid-1957 and was then designated as the 26th of July Movement's director of public relations outside of Cuba. In this capacity one of his main activities was to assure Americans that the Castro movement was democratic and non-Communist. Before leaving Cuba, Llerena had been asked to prepare a document setting forth the ideological bases of the revolution. The result was a booklet entitled *Nuestra Razón*, and the story of what happened to this "manifesto" is a chronicle of Llerena's growing feeling that he, and other liberal Cubans, were being used by Castro. The publication circulated throughout the United States and Latin America, but it was never accepted by the leaders of the movement. Significantly, the "manifesto that was not," as Llerena calls it, was never acknowledged by Castro. This document consisted of the "traditional philosophy of freedom and human dignity" (p. 101).

Llerena's increasing misgivings about the drift of the revolutionary leadership in 1958 caused him to drop his public relations work for the movement. He did not oppose Castro, and returned to Cuba in 1959 to write for several publications. Within a few months Castro began to publicly denounce him, and after the publications were closed or taken over by the government, Llerena once again became an exile.

This book documents the significant role that nonradical liberals played in the development of the revolutionary movement. It also provides some insight into the devious nature of a movement and a leader that came to power "under the borrowed banner of a democratic, ethical reform movement, cheered on by a joyfully confused populace" (p. 256).

ROBERT FREEMAN SMITH
University of Toledo

MARIO RODRÍGUEZ. *The Cádiz Experiment in Central America, 1808 to 1826*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xii, 316. \$18.00.

Have the Central American people ever appreciated the values of representative government? Did they once attempt to make the most of the advantages they enjoyed from their relatively peaceful transition to independence? Mario Rodríguez answers both these questions with a hearty affirmative. Central Americans did appreciate those values, which they had helped introduce to Spain, and they did attempt to put them into practice on the isthmus. They failed because geography and economics worked against them. Too many lacked the spirit of unity; too few cared to pay its price.

Today Costa Rica is the only Central American country where an opposing party can win without compromising its position in advance. But from 1808 to 1826 each province on the isthmus included its share of persons who expressed themselves well in debate and preferred to work through constitutional processes to achieve their programs. Early in his career Rodríguez became interested in these men, particularly those of liberal sympathies. He hoped to write an in-depth biography of José Francisco Barrundia, one of the most distinguished. He produced instead this biography of ideas, adopted in part by the Spanish Cortes of 1810-13 in Cádiz and carried to fruition in two Central American declarations of independence and the first Central American constitution.

The isthmus retains the spotlight in this account, even in the two chapters covering Cádiz. Rodríguez emphasizes the controversies that arose there between Europeans and Americans and demonstrates that the six representatives of the isthmus consistently took the American side. Disappointed in their hopes for a limited monarchy through their experiences with both Spain (1814-20) and Mexico (1822-23), the lawyers and enlightened clergy of Central America eventually turned to republicanism. Rodríguez follows their ample documentation, which he pursued rigorously in many locations. The four most useful items were

the published *Diario de las discusiones y actas de las Cortes*; the *Libros de Cabildo* (Guatemala City); the *Actas de la Ex.ma Junta Provisional Consultiva* (and de la *Ex.ma Diputación Provincial*) de Guatemala; and the *Actas, Asamblea Nacional Constituyente*. The Archivo General de Centro América houses the last three.

Central American history of this period (and through the dissolution of the federation) presents one especially puzzling aspect. Allies became enemies and foes became friends in a bewildering pattern of interpersonal relationships. Rodríguez begins to untangle the conundrum, explaining motivations with care and emphasizing the overwhelming importance of regionalism in the search for political comrades. This type of explanation needs to be further developed in order to explain how liberalism came to be equated with union in Central America while tending toward separatism elsewhere.

The reader will appreciate the author's conversational style. The historian will like the chatty and informative "Essay on Authorities" and description of the "Archives and Other Repositories of Primary Materials." Friends may lament that the endnotes, full as they are, contain relatively less of the author's personality.

FRANKLIN D. PARKER
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JOHN LEDDY PHELAN. *The People and the King: The Comunero Revolution in Colombia, 1781*. Madison: University of Wisconsin Press. 1978. Pp. xix, 309. \$25.00.

In the introduction to this volume the late John Leddy Phelan explained that it "grew out of" his much acclaimed and influential earlier work, *The Kingdom of Quito in the Seventeenth Century* (1967). While that study had been an attempt "to explore the inner workings of the colonial bureaucracy and to examine those conditions which enabled the administration to conciliate tensions and conflicts," this later work "looks at the other side of the coin"—the kinds of conditions under which the system would break down. The revolution of the *Comuneros* in New Granada was certainly an instructive example to choose, both because of its scale and because it involved a significant degree of participation by all elements of the population except black slaves. Phelan's account is absorbing in detail and generally convincing in interpretation. Unfortunately, it did not receive good editing. The overall organization is logical and easy to follow, especially if one has some prior knowledge of the subject, but the structure at sentence- and paragraph-level frequently is not.

The study does not claim to present major revelations with respect to the events themselves. Indeed Phelan repeatedly acknowledged his debt to such Colombian historians of the Comunero movement as Pablo Cárdenas Acosta, and he had earlier presented (in articles in the *Boletín de Historia y Antigüedades*) most of the book's fascinating though somewhat speculative contribution on the relationship between the rebellion that erupted in Socorro and disgruntled creole circles of Bogotá. On the other hand, the footnotes are brimming with archival citations on all aspects of the story, and if Phelan did not significantly revise the main narrative he did enrich it with many illustrative details and insights. His handling of the complexities of social position and the different aims of social groups, all too often neglected or distorted, is especially commendable.

Phelan was perhaps carried away at times in his admiration for the principal Comunero captain, Juan Francisco Berbeo, to whom he attributes "uncanny ability," and he might be faulted for not taking a more skeptical look at the traditional figure of 20,000 for the Comunero army encamped at Zipaquirá. But he was skeptical (though ever courteous) about the claims of various Colombian historians that the rebellion of 1781 was either an early attempt at independence or an abortive social revolution. Phelan saw in it instead an attempt to restore a traditional "unwritten constitution" of government by negotiation and compromise that the "technocratic" agents of Charles III, particularly the regent-visitor general Juan Francisco Gutiérrez de Piñeres, had violated with ill-advised fiscal and bureaucratic innovations. The archbishop-viceroy Antonio Caballero y Góngora then succeeded in pacifying New Granada, according to Phelan, by artfully devising a "synthesis" of "unwritten constitution" and technocratic innovations that left no party fully satisfied but none wholly frustrated.

DAVID BUSHNELL
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RICHARD E. SHARPLESS. *Gaitán of Colombia: A Political Biography*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1978. Pp. vii, 229. \$11.95.

Jorge Eliécer Gaitán, Colombia's only really effective populist leader, first became prominent in 1929 when he led congressional denunciations of the government's role in suppressing strikes against United Fruit. Of lower-middle-class origins, Gaitán was always an outsider in the highest conclaves of Liberal politics. Nevertheless, by championing reforms appealing to both peasants and

urban workers, he rallied so much mass support that party leaders constantly were forced to come to terms with him. In 1946, when Liberal leaders refused to designate him as their presidential candidate, he ran independently, permitting victory by the minority Conservative party. Consequently, by 1948 the Liberal oligarchy reluctantly had to recognize him as a party leader. On April 9, 1948, Gaitán was assassinated on the principal street of Bogotá. Within an hour, thousands of the frustrated poor, enraged and desperate at the death of the leader who represented their only political hope, were tearing down and burning most of the central portion of the capital. The government tottered as President Mariano Ospina Pérez was virtually imprisoned in his palace and the political elite in general cast about for ways to restore authority to a political structure that for much of the populace had lost all legitimacy. Although Gaitán's political movement died with him, he and his violent death continue to haunt the political elite while the mass of the people remain alienated.

Gaitán clearly is an attractive subject. One might expect the first English-language political biography to be the work of some hot-eyed romantic radical, intent on bathing his hero in purple prose and condemning the evil oligarchy with righteous indignation. Instead, Richard E. Sharpless, while clearly sympathetic to Gaitán and his objectives, presents a serene, judicious, well-balanced work, a political biography without blinders.

The book analyzes Gaitán's individual motivation and actions, describes his role in Colombian politics, and, finally, discusses him as an example of Latin American populist politics in general. The analysis of Gaitán's personal formation is a bit mechanical ("from his mother . . . practical zeal . . . from his father . . . speculative aspirations" [p. 41]), but the general argument is convincing. Sharpless contends that Gaitán's lower-middle-class origins inspired in him both a strong passion to redeem the poor and a personal desire to fight his way into the political and social establishment. The author argues that these tendencies, apparently in conflict, blended to make Gaitán a particularly effective leader. Gaitán's shifting political course (in and out of the Liberal Party), viewed as opportunism by some, is seen as politically realistic by the author. Gaitán's desire to celebrate his success with fine clothes, large cars, club memberships, and, ultimately, an upper-class wife strengthened his popular appeal since the nether classes could identify with and delight in that success. In general the author's interpretation reveals an excellent understanding of Colombian politics and society—upper-class domination of the two traditional parties, middle-class hunger for the appearance of upper-class respectability, and the

consequent importance of patronage and prevalence of opportunism. Finally, he provides a sound analysis of Gaitán's political significance as one who threatened to replace the vertical cleavages fostered by the elite with a populist horizontal cleavage.

The author's research into published materials is thorough, but he might usefully have interviewed more than eight individuals. Because of his concentration on Gaitán's personal style, Sharpless provides too little discussion of others in his movement. Gaitán's most important collaborators are simply described as "middle class." One would like to know more about them, how they became connected to Gaitán, and how they differed socially from others in politics. Sometimes the political context is not sketched adequately. The author seems to ascribe a significant change in land tenure (1932-40) almost entirely to Gaitán's movement of 1932-36, without mentioning the government's land reform law 200 of 1936. The author also translates Colombianisms from the Spanish so literally that their real meaning will be lost on English-language readers not familiar with Colombian usages. Despite some weaknesses, however, this is a competent piece of research and analysis and a welcome contribution to our understanding of twentieth-century politics in Colombia.

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PAUL W. DRAKE. *Socialism and Populism in Chile, 1932-52*. Urbana: University of Illinois Press. 1978. Pp. xii, 418. \$15.00.

For all that has been written about the *Unidad Popular* experience in Chile, little historical insight into the formation, role, and character of the Socialist Party in the Chilean political system has been offered. With the appearance of Paul Drake's *Socialism and Populism in Chile, 1932-52*, we finally have a careful, well-documented, and sensitive interpretation that does justice to the complexity and chameleon-like behavior of Chilean socialists.

The period immediately following the Ibáñez dictatorship (1927-31) was a critical interlude in Chilean politics, marking a transition from the instability of the post-World War I years to four decades (1932-73) of uninterrupted formal democracy. More fully than any previous study, Drake's work describes and documents the basis of the political realignment in Chilean politics of the 1930s that provided the foundation for forty years

of political democracy. Drake's analysis of the period 1931-33, in which he examines the relationship between socioeconomic change in Chile and the behavior of the traditional, middle-class reformist, and Marxist political parties, provides a benchmark for future historical research in Chile on the middle decades of the twentieth century.

Analysis of the 1931-33 period is followed by detailed treatment of the Popular Front experience, the "decline" of the Socialist Party (1942-52), and a balanced review of the trajectory of Chilean socialism in the last two decades. In a striking paragraph near the end of the epilogue, Drake succinctly describes the limits placed by Chilean formal democracy on fundamental reforms and captures the dilemma of four decades of contradictions and internal divisions among Chilean Marxists and reformers: "In the Popular Front, the Marxists gained access to the political upper echelons, but at the cost of putting a lid on worker organizations and programs. In the Popular Unity, they mobilized, or allowed the mobilization of, the workers behind massive social changes, but at the cost of being disbarred from government" (p. 326).

If there is a weakness in Drake's book, it is a weakness that reflects the lack of clear self-definition of Chilean socialism itself and of the varieties of "populism" in Europe, North America, and Latin America. Seeking to place Chilean socialism in the context of populist and Marxist movements in Latin America and elsewhere, Drake tells us of "socialism in populist trappings" (p. 71), a middle-class breed of socialism (p. 76), populist socialism (p. 83), liberal populism (p. 84), socialist populism (p. 84), and the middle classes and their populist followers (p. 87). We are also introduced to "the . . . acculturation of populism and Socialism" within the Chilean political system (p. 113).

The lack of clear definitions—both by the participants themselves and by the author—gives rise to a certain terminological confusion. But then the world of Chilean politics as well as the character of populist-socialist movements in Latin America, Chile, and elsewhere hardly lends itself to easy, precise labels. International political and economic forces, domestic politics, personalism, and the idiosyncrasies of different political systems have all affected the character of populist and socialist movements around the world. Drake's book considers all these factors and more; it represents an important addition to our knowledge of Chilean political development.

BRIAN LOVEMAN
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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

WILLIAM H. MCNEILL and RUTH S. ADAMS, editors. *Human Migration: Patterns and Policies*. Bloomington: Indiana University Press, in association with the American Academy of Arts and Sciences. 1978. Pp. xviii, 442. \$22.50.

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MALCOLM TODD, editor. *Studies in the Romano-British Villa*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. 244. Paper \$14.50.

WARWICK RODWELL, Rivenhall and the emergency of first-century villas in northern Essex. D. S. NEAL, The growth and decline of villas in the Verulamium area. J. P. WILD, Villas in the lower Nene Valley. DAVID E. JOHNSTON, Villas of Hampshire and the Isle of Wight. ROGER GOODBURN, Winterton: some villa problems. H. J. M. GREEN, A villa estate at Godmanchester. D. J. SMITH, Regional aspects of the winged corridor villa in Britain. J. T. SMITH, Villas as a key to social structure. JOHN HADMAN, Aisled buildings in Roman Britain. MALCOLM TODD, Villas and Romano-British society. D. F. MACK-

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TO THE EDITOR:

It is assumed that most historians welcome the work of colleagues who seek new approaches and new interpretations covering a given period or aspect of history. Flaws may be found in such endeavors or their conclusions may clash with older or conventional viewpoints, but these are precisely among the consequences that inevitably stem from creative efforts. In that context, the article entitled "American Imperialism: The 'Worst Chapter' in Almost Any Book" by James A. Field, Jr. *could* have been accepted as a genuine contribution toward blazing new paths in the historiography of the era of the Spanish-American War.

Unfortunately, however, neither the flaws nor the conclusions in Field's article are the fundamental issue but rather his flippant treatment of the subject of his essay—"American Imperialism." Field conceded that he "tried very hard not to use the word outside of quotation marks or paraphrase." It was "undesirable," he continued, "to begin with an assumption of an undefined 'American Imperialism': better describe the attitudes, the aims, the capabilities, and the events, and then see what they add up to" (page 682).

Grammatically, this rationale reads well but it stands elementary logic on its head. On what grounds can Field justify surrounding American imperialism with quotation marks because it is an "assumption . . . undefined" when he deliberately avoids what he should have done in the first place, namely, to define modern imperialism. Incidentally, if he had denied the reality of imperialism, his article would have been redundant.

By discoursing on an assumed, "undefined" American imperialism without probing the essentials of modern imperialism, Field found himself in the strange position of writing about a subject without analyzing its meaning! Indeed, the essay itself bears this out, for, while it cites some specific bits of interesting information, the total yields a superficial exposition—that is, Field's labors are devoid of in-depth analysis. Field's article, therefore, may shine with glittering prolixity but it withers and becomes worthless as a scholarly attempt to clarify or shed new light on the nature of United States foreign policy and activities during the last decade of the nineteenth century.

HERBERT KRIEDMAN
Nassau Community College

PROFESSOR FIELD REPLIES:

I naturally regret that my article should have so exercised Herbert Kriedman. But, since my concern is with what happened and why, and his appears to be the manipulation of assumptions, the circumstance was perhaps unavoidable. Indeed, if he had not been so preoccupied with constructs, he might have noticed that I was not writing about "American imperialism," but rather about some of the events of the period and how they have been misunderstood, mishandled, invented, or overlooked in much of the current literature.

JAMES A. FIELD, JR.
Swarthmore College

TO THE EDITOR:

I do not think that David Noble's review of my *Appointment at Armageddon: Muckraking and Progressivism in the American Tradition* (AHR, 82 [1977]: 198) was intended to derogate a book twenty years in the making, but its implications demand clarification in a field—Progressivism—that has been under attack not only by Gabriel Kolko, whom Noble mentions, but such others as Richard Hofstadter, Sam Bass Warner, Jr., C. Vann Woodward, and John G. Sproat.

Noble put it that my concern has been "to restore vitality to the progressive tradition." This is not the case. Should the evidence warrant derogation, Progressivism should receive it. My book notices as it unfolds the demeaning accounts offered respecting Theodore Roosevelt, Robert LaFollette, David Graham Phillips, and others and also notices the charges of "racism" and the alleged sell-outs to big business. Readers are free to judge my evidence against that of others.

But do I indeed give "brief" accounts of Anne Royall and William Lyon Mackenzie and, I suppose, many others not mentioned? What other account is there of Mackenzie's American career and his pioneer muckraking sensations? In what graduate seminar has he been treated and with what results? The fact is that Mackenzie's muckraking role was sunk and lost, in Canada as well as the United States, and left a hole in continuity, to say nothing of his content. My account introduces the subject: it would have destroyed proportion to have written more. Anne Royall's career has actually been available (see Heber Blankenhorn, "Grandma of the Muckrakers," *American Mercury* [September 1927], 87-93). It has recently been exploited by women activists; it has not previously been integrated in the mainstream, or any stream, of reform.

Noble complained that I compared the effects of the Civil War on subsequent reform with the negative effects of World War I but that I did not explain why. In fact, the purpose of the chapter was to interpose the Civil War between the famous era of reform and postwar developments. Armageddon took place in 1912. I emphasized developments to that date; developments following explain its significance, they do not dilate on what should be the matter for another book.

LOUIS FILLER
Antioch College

TO THE EDITOR:

I would like to respond to Robert E. Brown's review of my book, *The American Colonies from Settlement to Independence* (AHR, 82 [1977]: 1058). May I assure readers of the review who might otherwise have been interested in reading my book but recoil

at the thought of a further dose of "conflicts of interpretation among New Left and Consensus historians" or "the injection of ideology" that these characterizations of it rest more on Brown's anxiety to expound and defend his own theory of "colonial middle-class democracy" than on the work itself. This anxiety, in fact, permitted him to write only half a sentence on the quite considerable portions of my general survey that have no bearing on these matters: "When Simmons avoids the injection of ideology, he produces some good history. . . ." This type of reviewing is not unknown, but one particularly regrets its appearance in a leading journal of the American historical profession.

Why Brown should indeed claim that failure to subscribe to his viewpoint on early American history is evidence of ideological commitment to the "left" is another question. But, if the use of terms like "deference" or "deferential"—Walter Bagehot arise and defend yourself!—or "Quaker oligarchy" or "New York's ruling class" are evidence, as Brown asserts, of "New Left" sympathies, then the "New Left" must be rejoicing at its vast powers of recruitment. But my analysis, of course, might be thought to be quite consistent with a highly conservative viewpoint, such as that of the late C. K. Shipton. Was it not J. R. Pole who generalized the ideas associated with the concept of "deference" in 1962?

Brown's ideological red herrings are also interwoven with assertions that I fail in factual accuracy. He stated that I cited (and by implication disregarded) Franklin and Chastellux on the "middle-class democratic nature of colonial society." Where? Chastellux is cited on frontier land-clearing, Franklin on several topics, but not on Brown's obsession. Brown also made much of my treatment of loyalist numbers. Here again, the inaccuracies are his. He transmuted figures for loyalist *exiles* (sixty to one hundred thousand) into figures for *total* loyalists, disregarded the modern estimates of loyalist numbers that I provided, and ignored my warning of the virtual impossibility of calculating loyalist numbers (page 375). This especially irritates since my book was reviewed and not found wanting by a leading historian of the loyalists! So much for Brown's caveat that my "treatment of patriots and loyalists provides a particularly good example of the need for historians to get their facts straight before they interpret them."

I hope I do not write intemperately; but, leaving aside for the moment Brown's unfounded ideological strictures, no author likes to find a large portion of a review of his book devoted to criticisms of "errors"—invented, for whatever reason, by the reviewer.

R. C. SIMMONS
University of Birmingham

TO THE EDITOR:

I wish to discuss certain statements made by Elizabeth Read Foster in her review of my *Francis Bacon: A Political Biography* (*AHR*, 82 [1977]: 1241). I disagree with her contention that my book lacks "a concrete description of Bacon's political skills, first as MP and later during his brief role as presiding officer of the House of Lords." My study stresses in detail Bacon's role in the Lower House from 1581 to 1614, and it shows how he emerged as a supporter of the crown during the sessions of 1597 and 1601 while maintaining the trust and respect of his colleagues. His skill as mediator in some of the key issues (the Goodwin-Fortescue case and the question of the Union) for the session of 1604 is carefully discussed, as is his attempt to persuade the House to accept a plan of union with Scotland in 1607. I have stressed how he attempted delicately to advance the crown's interest without engendering the hostility of his colleagues in the House. I believe I have demonstrated in detail Bacon's skills as a tactician during this sensitive period in the history of Jacobean Parliaments. In 1610, as solicitor-general, Bacon realized that his role had changed and that he might find himself in sharp conflict with the House.

With respect to any "role" Bacon might have played as a tactician in the House of Lords in 1621, I did suggest (page 138) that the chancellor approached the session "naively," underestimating that Parliament's exploitation of the monopolies issue might expose him to attack. The grievance against monopolies was raised in committee at the very outset, and rather quickly a foundation for a case against Bacon began to develop. I showed clearly how the chancellor did not realize the danger and how, as Robert Zaller has put it, Bacon "stood isolated" before any "formal charge had yet been placed against him." How, then, was Bacon to play an active role in "directing debate in the Upper House?"

Elizabeth Read Foster also felt that my contention that Bacon was impeached because he was a convenient scapegoat constitutes a generalization that suggests "an overly simplistic view of Parliament in the Stuart period." I have treated the impeachment carefully and, I feel, quite thoroughly. I have shown how the case against Bacon—far from being premeditated—evolved out of the business of the session. Once Bacon's vulnerability became apparent, however, individuals such as Edward Coke, Lionel Cranfield, and Robert Phelps moved quickly to build a strong case against him. The view that Bacon was essentially a political victim may not be new, but to my knowledge it has not been successfully refuted. If a less "simplistic" interpretation is in order, then it should be explained in the review.

Elizabeth Read Foster is a respected scholar, and I welcome some of her comments. While I recognize that reviews are limited, I would find it helpful if references to supposedly "overly simplistic statements" and "errors in interpretation" were explained.

JOEL J. EPSTEIN
Olivet College

PROFESSOR FOSTER REPLIES:

Joel Epstein does not agree that he failed to give "a concrete description of Bacon's political skills, first as an MP and later during his brief role as presiding officer of the House of Lords." Certainly, he described in detail *what* Bacon did or tried to do. What he did not tell us is *how* Bacon worked in Parliament (that is, what political skills he used). In the Lower House, did Bacon function chiefly in committee or on the floor? Did he speak first or last in debate? Did he rely on his verbal facility or could he bring other influences to bear? Did he work with a particular group of members or operate alone? In the Upper House, Bacon directed proceedings and made careful arrangements to handle a critical session. He resorted to little parliamentary tricks or adjourned the House to postpone undesirable debate. Sometimes he solicited the aid of the king, the prince, the duke, and the lord treasurer. Even after charges had been brought against him, he worked in the same way to try to persuade the House to ameliorate his sentence. (See Frances Helen Relf, ed., *Debates in the House of Lords, 1621, 1625, 1628* [1929], 3-7, 11, 21; and James Spedding *et al.*, eds., *Letters and Life of Francis Bacon*, 7 [1874]: 190-92, 241).

The errors in interpretation pertained, as I said in my review, to the position of the attorney-general in the Lower House, the bill for continuation of statutes, and statements concerning Puritanism and the religious situation in Scotland. It is questionable, after Patrick Collinson's work, whether one can call Elizabethan Puritanism "an embryonic movement." It is also questionable whether English religious problems were "far more complex" than those of Scotland (pages 31, 62, 69, 104).

Joel Epstein asks me to explain what I meant by referring to his "overly simplistic view of Parliament in the early Stuart period." The examples I gave were Epstein's remarks concerning impositions (pages 86-87) and his conclusions concerning the significance of Bacon's disgrace. In both, he presented an interpretation of events that historians are now abandoning, an interpretation that pits Parliament against the king. At some point early in 1621, Bacon is said to have told James that "those that will strike at your Chancellor, it is much to be feared will strike at your crown."

Epstein took this self-serving statement as the theme of his chapter on Bacon's fall; and consequently he came to the conclusion that Bacon fell "because he stood as an ideal victim for a Parliament looking to vent hostility against the Crown. He was an assailable symbol of royal power" (pages 154-55). Here the assumption, unexamined and unsupported by the evidence, is that Parliament *was* at this time hostile to the crown. To examine the quality of justice in the courts, to criticize referees who had passed unpopular and possibly illegal patents, or even to convict a judge of bribery and corruption or to charge an officer of the crown with wrongdoing was not necessarily to assail royal power or to "vent hostility against the Crown." In speaking to the House of Lords on March 10, 1621, the king had specifically dissociated himself from Bacon and others under question. "As for the things objected against the Chancellor and the Treasurer," James had said, "I leave them to answer for themselves and to stand and fall as they acquit themselves, for if they cannot justify themselves they are not worthy to hold and enjoy those places they have under me." (Lady de Villiers, ed., *The Hastings Journal of the Parliament of 1621*, viii-ix, 27-30). Both sovereign and subjects had long held that one of the proper functions of Parliament was to acquaint the king with the grievances that afflicted the realm. To understand the great reforming Parliament of 1621 and the fate of Bacon, we need an interpretation that allows a broader view of the institution and of its role in English political life than Joel Epstein has given. In his conclusions, he seems to me to oversimplify what had happened, and to forget the complexities of the situation that he had previously carefully described.

ELIZABETH READ FOSTER
Bryn Mawr College

TO THE EDITOR:

Permit me to take issue with one point in Linda Gordon's review of my *The Horrors of the Half-Known Life: Male Attitudes toward Women and Sexuality in Nineteenth-Century America* (*AHR*, 82 [1977]: 1328-29): the allegation that my work is not original. (Limitations of space prevent the refutation of other serious mistakes.) Gordon stated that my book is "an extension and variation of substantial previous work by, for example, Peter Cominos, Carroll Smith-Rosenberg, and Charles Rosenberg." The relevance of Cominos's two articles, written in 1963 and 1970, is confined to British history. As for the relation of my work and that of Smith-Rosenberg and Rosenberg, Gordon has inadvertently reversed the indebtedness. And it is not a very happy story.

My dissertation was accepted by the University of California at Los Angeles in June 1968. It has the same title and substantially the same argument as my book. It demonstrates, among other things, that nineteenth-century medical journals are extremely useful sources for the social history of sex roles. In December 1968 at the annual meeting of the American Historical Association, I applied for a temporary position at the University of Pennsylvania to replace Charles Rosenberg, then going on leave. He asked for a copy of my dissertation. In turning it over to him I met his wife of that time, Carroll Smith-Rosenberg, who told me she was beginning work on the history of child-rearing. Rosenberg returned my dissertation to me in the spring of 1969.

Subsequent to this exchange I and the Rosenbergs began to publish work about the history of sex roles in America. I presented papers based on my dissertation at the meeting of the Organization of American Historians in April 1971 and at the meeting of the American Historical Association in December 1971; the former, "The Spermatic Economy," was published in the summer of 1972 and reprinted in Michael Gordon, ed., *The American Family in Social-Historical Perspective* (1973). I submitted the book manuscript to Harper and Row in December 1974.

Please compare the dates of these events—1968 to 1974—to the dates of the four Smith-Rosenberg and Rosenberg articles to which I assume Linda Gordon refers: Winter 1972, Winter-Spring 1973, May 1973, and September 1973. These articles acknowledge my work in the following manner: the article by Smith-Rosenberg published in the winter of 1972 cites my dissertation (note 45) as "a speculative psychoanalytical approach" along with an article by Ann Douglas Wood, although the citation interestingly gives the dissertation's date as 1969 (the year I loaned it to Smith-Rosenberg's husband) rather than 1968 (its actual date); and both articles published in 1973 include citation to my dissertation in their omnibus first footnotes. But, when Smith-Rosenberg published her historiographical survey, "The New Woman and the New History" (1975), she ignored my work in favor of other historians' writings about sexuality.

In my book I did not refer to the Smith-Rosenberg and Rosenberg articles because, in my view, they added little but jargon and additional evidence to my argument and to that of Donald Meyer in his *The Positive Thinkers* (1965). None of these remarks is to suggest that we do not stand on the shoulders of our predecessors; in fact, my own shoulders are already a little bruised.

G. J. BARKER-BENFIELD
Davis Center for Historical Studies,
Princeton University

TO THE EDITOR:

I must respond to Bentley Gilbert's comments on my book, *Lloyd George and Foreign Policy*, vol. 1: *The Education of a Statesman, 1890-1916* (*AHR*, 83 [1978]: 441-42), not because they are disparaging but because they do not constitute a review. He has, in other words, deliberately ignored his assignment.

Several years ago, several of us agreed that a comprehensive biography of David Lloyd George was, at that point, a premature undertaking. The Randolph Churchill-Martin Gilbert project on Winston Churchill did not seem to be a compelling model. I decided to work on foreign policy questions—a perfectly legitimate approach with a lengthy pedigree and in a sound enough tradition. We felt, I suppose, that the great synthesis would come in its own good time. Perhaps Bentley Gilbert is the man for the task; perhaps he has the project already under way? Clearly, he rejects our contributions as trivial and says so at some length. He demands a holistic approach as the only valid one: Historians must present the whole man, in all of his complexity, acting over all issues.

So be it. Gilbert has made his point, but what of my book? Where are the comments that an author has the right to expect—on the research, the analysis and interpretation, the command of the subject, the quality of the writing, the contribution to the field, and so on? By any standards of reviewing, Gilbert has failed the editor, the author, and the reader.

I prefer not to take issue with the very few comments Gilbert did make directly about the book. Perhaps the problem is that his expertise lies elsewhere than in the realm of dissent, defense policy, and diplomacy. If that were to remain true, I

would expect a more appropriate reviewer for my second volume.

MICHAEL G. FRY
University of Denver

TO THE EDITOR:

I would like to offer one factual correction to Richard Dekmejian's review of Dickran Boyajian's *Armenia: The Case for a Forgotten Genocide* (*AHR*, 81 [1976]: 916-17). The "relevant sections of the Ottoman Archives of Istanbul" are not closed for foreign scholars who are able to read the language of the documents, Ottoman Turkish, nor have they ever been closed to qualified scholars since the administrative changes in the archives after 1961. Indeed, one American scholar recently received permission to use these very sources about Ottoman Armenia.

ALAN W. FISHER
Michigan State University

TO THE EDITOR:

Concerning the review of Keith L. Bryant, Jr., *History of the Atchison, Topeka, and Santa Fe Railway*, by Merl E. Reed (*AHR*, 81 [1976]: 1238), there is one point that can be clarified. The reviewer states, "In regard to construction, I searched modern gazetteers in vain for the Kaw River (pages 8, 15)." There is no problem. The Kaw is another name for the Kansas River, which flows through Topeka and Lawrence and into the Missouri River at Kansas City. Fifty years ago students at Washburn College, before it became a municipal university in Topeka, referred humoristically to their institution as the "Washboard on the Kaw."

E. FREEMAN GOSSETT
Fullerton College

Recent Deaths

MYRON GILMORE was born in Walpole, Massachusetts on July 4, 1910. He died in Cambridge on October 27, 1978. Aside from a few years of military service in Washington during the Second World War, his education and his professional career were closely tied to the two leading institutions of higher learning of his native state. He was graduated *summa cum laude* from Amherst College in 1932. In 1962, his alma mater recognized his achievement as one of America's most accomplished Renaissance scholars by awarding him its honorary degree of Doctor of Humane Letters. Had illness not intervened, he would have returned to Amherst in 1978 for a prolonged visit as Robert Frost Lecturer.

After leaving Amherst, Myron Gilmore took an M.A. at Harvard in 1933, and continued on for his Ph.D. in 1937. A revised version of his dissertation appeared in the Harvard Historical Monographs in 1941 as *The Argument from Roman Law in Political Thought, 1200–1600*. This early work gives evidence of many of his striking scholarly gifts: an ability to develop complex arguments in a graceful and economical style; a range of erudition far beyond the boundaries of a specific period, national culture, or historical specialty; an intellectual modesty that led him to demand rigor in his own work, while preserving a broad respect for alternative approaches. It also reveals a set of intellectual concerns that Gilmore pursued for many years thereafter: the continuity of the learned traditions of the ancient world into the late Middle Ages and early modern times; the development of the learned professions during the Renaissance and their impact on the great political and religious debates of the fifteenth and sixteenth centuries; and the contributions of such seminal minds as those of Bartolus, Erasmus, and Zasius to European jurisprudence and humanistic culture.

Gilmore's acclaimed course on the Renaissance and Reformation was for many years a Harvard institution, justly celebrated as a comprehensive and balanced introduction to a richly complex period. Together with numerous graduate seminars, it was the staple of a teaching career that

began with Gilmore's appointment as instructor in 1935. After returning to Harvard in 1946 from the Office of the Chief of Naval Operations, from which he was discharged as a lieutenant commander, Gilmore resumed his career as associate professor until 1954, when he became professor. From 1957 to 1962, he served as chairman of the department of history. At the time of his death, he was Gurney Professor of History and Political Science.

Without being identified with a particular thesis or theory, Gilmore's writings established his reputation as a most learned and judicious interpreter of early Europe. *The World of Humanism, 1453–1517* (1952), his contribution to the famous Langer series, has been used by generations of undergraduate and graduate students as a *vademecum* to a notoriously problematical era. Over a quarter century after its appearance, it still enjoys a wide following, and remains remarkably current in its outlook and approach. In later works, such as *Humanists and Jurists* (1963), he probed the characters and the intellectual dilemmas of some of the Renaissance men he most admired.

Indeed, the title of Renaissance Man, while he would have rejected it out of hand, was one that Myron himself amply merited. As writer, lecturer, and university administrator he was exemplary. His devotion to his family was virtually Albertian. He rejoiced as much in nature as in art and was an imaginative gardener. He had every social grace—gilded Florentine aristocrats and dusty bibliographers felt equally at home in his company, for he drew all he met into a gentle and cultivated discourse.

All of these gifts came into play in new ways in 1964, when he began his term as Director of Villa I Tatti, the Harvard Center for Italian Renaissance Studies in Florence. Writing in the London *Times* (November 9, 1978), Michael Hirst and Nicolai Rubinstein note that this appointment, which lasted nine years, "opened a new and deeply-rewarding chapter in his life, in which the breadth of his interests, his passionate love of Italy, and his extraordinary human sympathy perfectly matched

the demands of his post. The new institute at Villa I Tatti, which owed its existence to Bernard Berenson's bequest, was, at the time of Myron Gilmore's appointment, still in its formative stage; Myron, together with his English wife Sheila, gave it a new impetus, and permanently impressed his stamp on it."

Under Gilmore's inspired leadership, the entire foreign academic community in Florence responded gallantly to the critical problems raised for the city's cultural patrimony by the disastrous flood of 1966. For his important role in the long and arduous program of restoration, Gilmore earned great esteem in Italy. He was inducted, as a Grande Ufficiale, into the Order of the Star of Italian Solidarity ("Stella della Solidarietà Italiana"). In 1976, the University of Florence conferred upon him an honorary Doctor of Letters. Returning to Florence in September, 1978, he had the satisfaction of receiving from former Fellows and Associates at I Tatti a two-volume festschrift, *Essays Presented to Myron P. Gilmore*. This compilation, together with the wide-ranging publications of his former students, reveals much about the range of his concerns as a scholar and teacher.

It is perhaps as a teacher and friend that Myron will be best remembered, beyond the circle of his devoted family. As a teacher, he sought to recognize and nurture the interests and abilities of each of his students. His method was to suggest rather than to direct, because he respected other minds,

and had no wish to impose a point of view. While his convictions were strongly held, he always believed, as he wrote in his essay on Boniface Amerbach, that "the attainment of Truth will be a slow process, the product of a continuing dialogue. . . ." For those who knew Myron Gilmore, this dialogue will always be enriched by his memory.

As a friend, Myron's generosity of spirit was boundless. The thoughtfulness of his hospitality, so well known to the many visitors who passed through the doors of 48 Mt. Vernon Street in Cambridge and of I Tatti, was matched by his congeniality as a guest. His kindness to students set him apart from, and above, many of the Harvard historians of his generation. At the same time, his humor and charm never led to a lowering of his intellectual sights and aspirations. A picnic in the Tuscan hills, a trip to the Palio in Siena, a walk along the Charles, a stroll through the gardens or fields of I Tatti, all could and did provide the setting for a continuous meeting of minds. His imposing learning, generously shared with all who sought it, was never imposed on them. And through an elegant personal style that may at first have seemed intimidating to some, there shone an open and democratic spirit utterly devoid of learned or social pretension. He exemplified the best qualities of the humanists he understood and explained so well.

WERNER L. GUNDERSHEIMER
University of Pennsylvania

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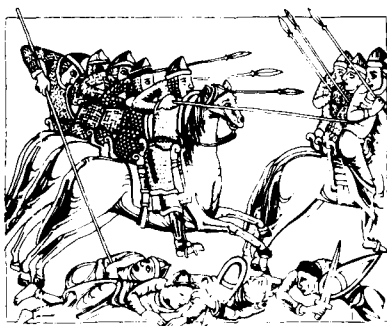
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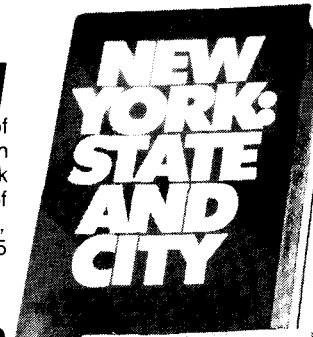
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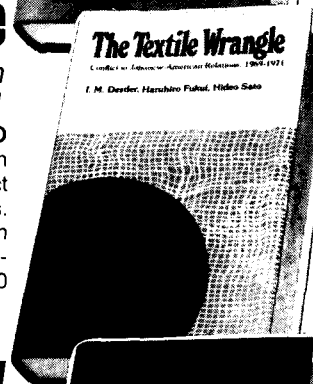


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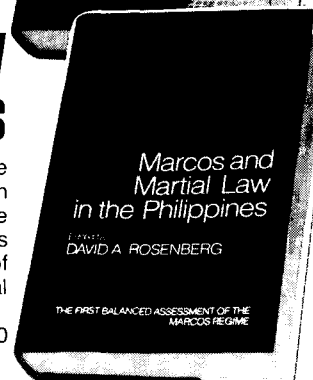
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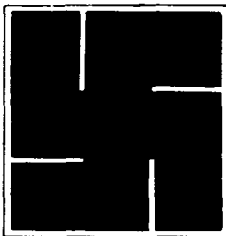
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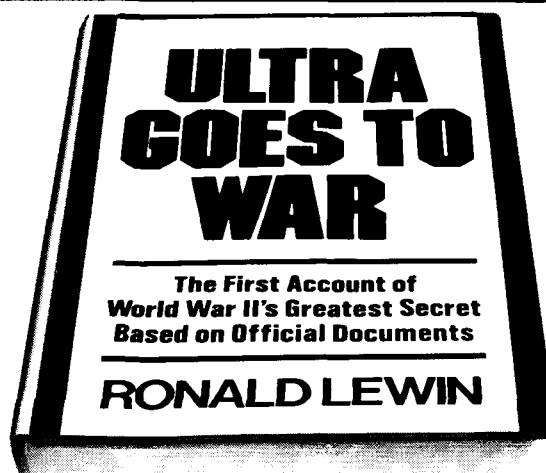
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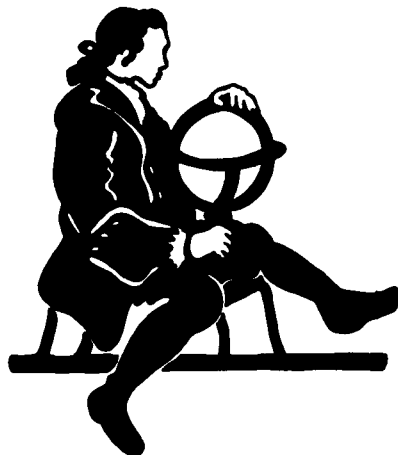
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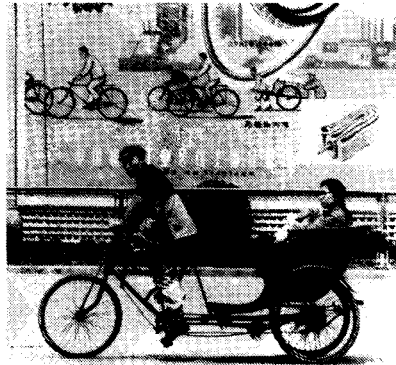
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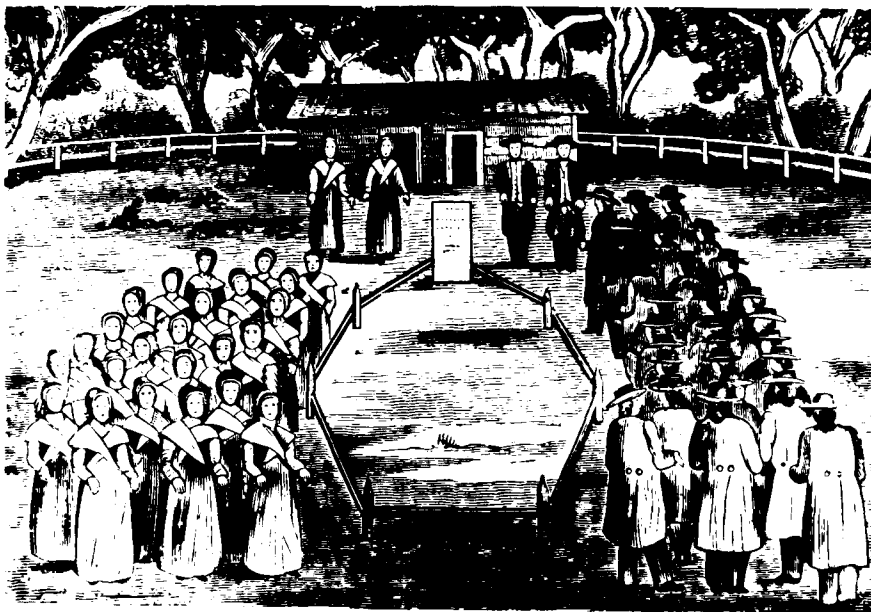
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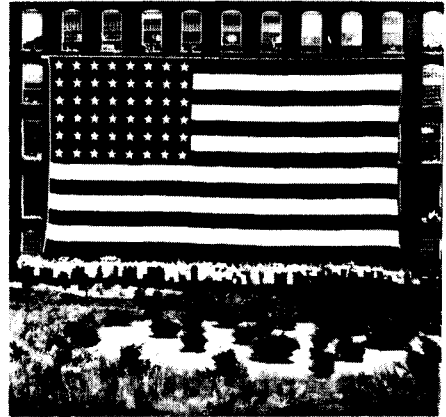


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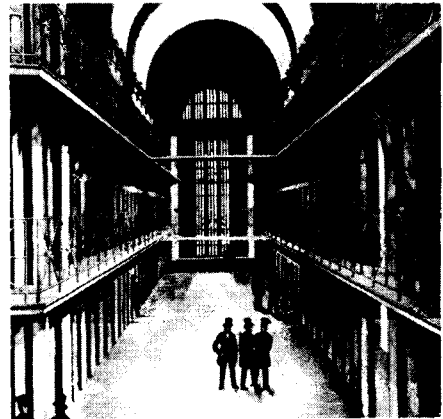
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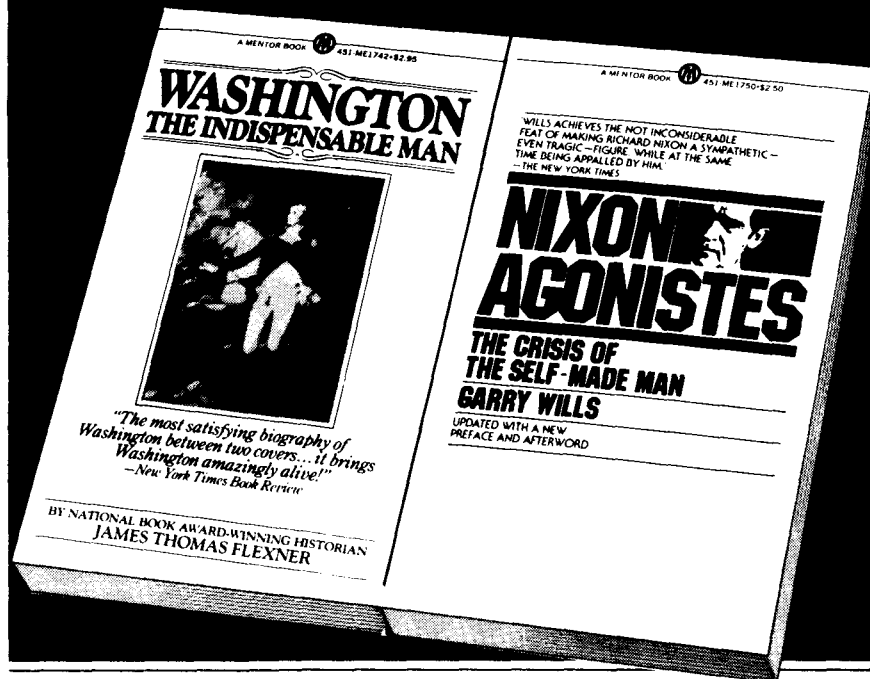
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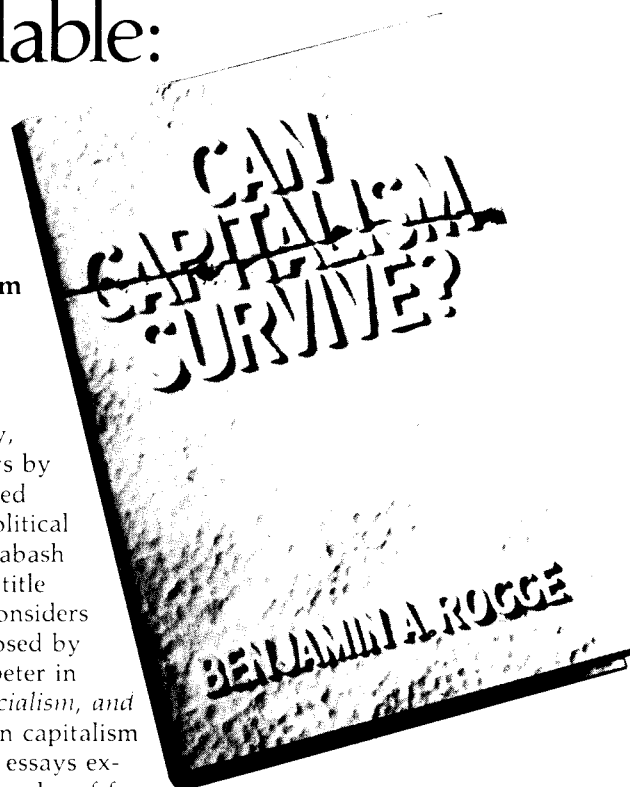


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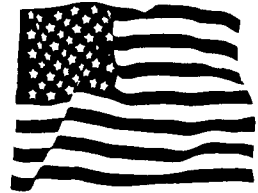
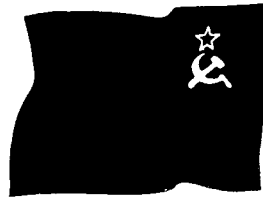
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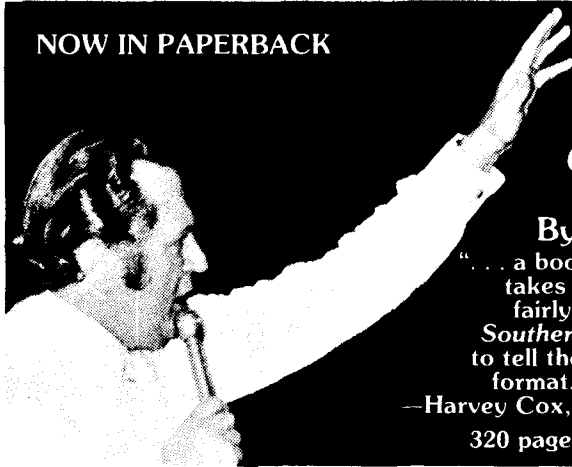
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